

EXPERIMENTS
AND
OBSERVATIONS
ON DIFFERENT KINDS OF
AIR,

AND OTHER BRANCHES OF
NATURAL PHILOSOPHY,
CONNECTED WITH THE SUBJECT.

IN THREE VOLUMES;
Being the former Six Volumes abridged and methodized, with many
Additions.

By JOSEPH PRIESTLEY, LL.D. F.R.S.

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MED. PARIS. CANTAB. AMERIC. ET PHILAD. SOCIUS.

V O L. III.

Fert animus causas tantarum expromere rerum,
Immensumque aperitur opus.

LUCAN.

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C O N T E N T S

OF THE

T H I R D V O L U M E.

B O O K V I I I.

EXPERIMENTS AND OBSERVATIONS RE-
LATING TO DIFFERENT ACIDS page I

PART I.

Of the nitrous Acid	-	-	<i>ibid.</i>
Sect. I. <i>Various Observations relating to the Process for making Spirit of Nitre, and of the Production of Air in the Course of it</i>	-	-	<i>ibid.</i>
Sect. II. <i>Observations relating to the Colour and Strength of the nitrous Acid, according to different Circumstances in the Process for making it</i>			9
Sect. III. <i>Of the Colour of the nitrous Acid</i>			15
Sect. IV. <i>On the Phlogistication of Spirit of Nitre</i>			31
Sect. V. <i>Of the Composition of Spirit of Nitre from dephlogisticated and inflammable Air</i>	-	-	43
Sect. VI. <i>Objections to the preceding Experiments considered</i>	-	-	54
Sect. VII. <i>Of Air produced by the Solution of vegetable Substances in Spirit of Nitre</i>	-	-	65
	A 2		Sect.

Sect. VIII. Of Air procured by the Solution of animal Substances in Spirit of Nitre	-	86
Sect. IX. Of the phlogisticated nitrous Vapour	100	
Sect. X. An Account of some Experiments made in consequence of an Attempt to confine the nitrous acid Vapour by Means of animal Oils	-	106
Sect. XI. Observations on the nitrous acid Vapour itself	-	115
Sect. XII. Of the Influence of Light on Vapour of Spirit of Nitre	-	126
Sect. XIII. Of the Impregnation of Water with phlogisticated nitrous Vapour	-	129
Sect. XIV. Of the Impregnation of Oils and of Spirit of Wine, with the nitrous Vapour	-	136
Sect. XV. Of the Impregnation of the Acids, &c. with the nitrous Vapour	-	144
Sect. XVI. Of Crystals formed by the Impregnation of Oil of Vitriol with phlogisticated nitrous Vapour	156	
Sect. XVII. Of the Action of nitrous Vapour upon some solid Substances	-	165
Sect. XVIII. Of the firing of inflammable Air in the Vapour of nitrous Acid	-	177
Sect. XIX. Of the Mixture of vitriolic and nitrous Acids	-	184
Sect. XX. Experiments on the Transmission of the Vapour of Acids through a hot Earthen Tube	195	
Sect. XXI. Miscellaneous Experiments on nitrous Acid	-	203
1. Of the firing of Paper dipped in a Solution of Copper in nitrous Acid	-	ibid.
2. Of		

CONTENTS.

2. *Of the firing of Gunpowder in different Kinds of Air* - - - 205
3. *Of a casual Production similar to Gunpowder* 206

PART II.

- Experiments relating to the marine Acid 208
- Sect. I. *Of the Colour of the Marine Acid* *ibid.*
- Sect. II. *Of the Impregnation of Marine Acid with various earthy Substances* - 215
- Sect. III. *Experiments relating to the Discharge of the Colour of various Solutions made by the Marine Acid* 221
- Sect. IV. *Of the Effect of a continued Heat on Spirit of Salt in Glass Tubes hermetically sealed* 227
- Sect. V. *Of the dephlogisticated Marine Acid* 235

PART III.

- Of the phosphoric Acid* - - - 240

BOOK IX.

- EXPERIMENTS AND OBSERVATIONS RELATING TO VEGETATION AND RESPIRATION - - - 247

PART I.

- Observations and Experiments relating to Vegetation - - - *ibid.*
- Sect. I. *Of the Restoration of Air, in which a Candle has burned out by Vegetation* - *ibid.*
- Sect. II. *Of the Restoration of Air infected with animal Respiration or Putrefaction, by Vegetation* 255
- Sect. III. *Experiments of Plants growing in vitiated Air in the Year 1777* - - - 273
- Sect.

Sect. IV. Of the Growth of Plants in dephlogisticated Air	276
Sect. V. Of the State of Air confined in the Bladders of Sea Weed	279
Sect. VI. Of the spontaneous Emission of dephlogisticated Air from Water containing a vegetable green Matter	282
Sect. VII. Of the Purification of Air by Plants, and the Influence of Light on that Process	293
Sect. VIII. Farther Observations on the green vegetable Matter with which many of the preceding Experiments were made	306
Sect. IX. Of the Production of the green Matter, and of pure Air, by Means of various vegetable Substances in Water	313
Sect. X. Of the Production of Air by Means of the green Matter from animal Substances	322
Sect. XI. Of the Property of the Willow Plant to absorb Air	331
Sect. XII. Of the Growth of the Willow Plant in different Kinds of Air	336

PART II.

Experiments and Observations relating to Respiration	348
Sect. I. Observations on Respiration, and the Use of the Blood	ibid.
Sect. II. Of the Consumption of dephlogisticated Air in Respiration	375
Sect. III. Of the Respiration of Fishes	382

Sect.

CONTENTS.

iv

Sect. IV. <i>Of the Diminution of nitrous Air in consequence of its being confined in a Bladder in certain Circumstances</i>	388
--	-----

BOOK X.

EXPERIMENTS AND OBSERVATIONS RELATING TO SEVERAL SUBSTANCES CONTAINING PHLOGISTON	395
---	-----

PART I.

Experiments on Charcoal	<i>ibid.</i>
Sect. I. <i>Experiments and Observations on Charcoal, first published in the Philosophical Transactions, Vol. LX. p. 211</i>	<i>ibid.</i>
Sect. II. <i>Experiments on Air from Charcoal</i>	414
Sect. III. <i>Of the Charcoal of Metals</i>	425

PART II.

Experiments and Observations relating to Mercury	432
Sect. I. <i>Observations relating to the Black Powder produced by the Agitation of impure Quicksilver</i>	<i>ibid.</i>
Sect. II. <i>Of the Agitation of pure Mercury in Water</i>	446
Sect. III. <i>Of the Effect of long continued Agitation on Quicksilver</i>	464
Sect. IV. <i>Experiments proving the great Volatility of Quicksilver</i>	470

PART III.

Experiments and Observations relating to Iron	480
Sect.	

Sect. I. On heating Iron in dephlogisticated Air, and afterwards in inflammable Air	-	480
Sect. II. Of the Quantity of inflammable Air yielded by Iron in its different States	-	491
Sect. III. Experiments on Finery Cinder		504

B O O K XI.

MISCELLANEOUS EXPERIMENTS AND OBSERVATIONS

Sect. I. Of the Electric Spark in different Liquids	ibid.
Sect. II. Of the conducting Power of certain Substances	512
Sect. III. Observations on Substances exposed to a long continued Heat	516
Sect. IV. Of the Colour given to Minium by Heat	531

B O O K XII.

OBSERVATIONS RELATING TO THEORY

Sect. I. Of the constituent Principles of the different Kinds of Air	ibid.
Sect. II. Of the Doctrine of Phlogiston	540
Sect. III. A more particular Answer to the Objections of the Antiphlogistians	554
Articles omitted	564

B O O K VIII.

EXPERIMENTS AND OBSERVATIONS RELATING TO THE DIFFERENT ACIDS.

P A R T I.

OF THE NITROUS ACID.

SECTION I.

Various Observations relating to the Process for making Spirit of Nitre, and of the Production of Air in the Course of it.

HAVING had occasion, as must have been seen, to make use of a considerable quantity of spirit of nitre, it will not be wondered at, that I should be desirous of thoroughly understanding the chemical process by which it is made, and of making it myself. This I wished to do, partly to save expence; but principally to examine with my own eyes every

VOL. III.

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thing

thing relating to it, and make what variations I should think proper in the process, in order to get the acid in the several *different states* in which I might have occasion to use it, without depending upon the report of any practical chemist.

Having acquainted Mr. Woulfe with my wishes, he was so obliging as to furnish me with a most commodious apparatus for the purpose, and to give me instructions how to use it.

From this time I have made so many distillations of this acid, and have varied the circumstances of it so much, that I now think myself qualified to teach others; and there are probably few persons who have had more experience in this particular process than myself. I think myself amply rewarded for the attention I have given to it, and I expect to derive still more advantage from the farther study of it.

As several of the observations which I have made upon this process are of considerable importance, and, as far as I know, are original, I shall give an account of the principal of them, especially so far as they tend to throw light upon the properties of this acid, which is so important an agent in every thing relating to the business of air.

It was first advanced by Mr. Woulfe, Phil. Trans. Vol. XVII. p. 178, and is now generally taken

taken for granted, that in this process, as well as in several other chemical distillations, there is a very great absorption of air. This always appeared to me very extraordinary, and to agree very ill with what I had myself observed concerning the production of air. Nitre itself, I knew, would yield air, and of a very pure kind; and though the addition of oil of vitriol would not *add* to the quantity, I saw no reason to think that it could *diminish* it. Had there, indeed, been any phlogistic matter in the retort, it would diminish the air contained in the vessels, and the fumes of the spirit of nitre itself have certainly the same effect. But this could not well exceed one fourth of the whole, and this by no means came up to what was generally supposed of the great consumption of air upon this occasion. And, though I knew that there must necessarily be a diminution, by phlogistication, of the common air contained in the retort and the receiver, &c. yet I could not help thinking it probable, that the air which would be *generated* from the materials would, in most cases, and especially if great heat was used in the process, and if it was urged to the utmost, more than supply that deficiency.

Accordingly, the first time that I had an opportunity of seeing this process from the beginning to the end, I added a small apparatus for receiving the

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air

air that might come from it, or at least to enable me to form a tolerable judgment whether there was, in reality, an excess either of *loss* or *gain* with respect to air in the course of it; and I saw enough to convince me that there was, at that time, an evident *production* of air.

The quantity of spirit of nitre that we made was about six ounces, with the proportion of six ounces of oil of vitriol to eight of nitre. The generation of air was observable in almost every part of the process, but more especially towards the end of it; and all the air that I caught appeared to be better than common air; the first part of it in the proportion of one and a half to one, and the rest of two to one. Also a considerable quantity came afterwards, which I did not catch, but only observed that when it was discharged into the open air, it was cloudy, as dephlogisticated air, when it is first produced, generally is. Indeed the superior quality of this air sufficiently proved that it was not the common air expelled by heat from the retort and receiver, but must have been produced from the materials employed in the process.

Though I was myself sufficiently satisfied with the result of this first experiment, I was willing to put an end to all possible doubt with respect to it, by repeating the process when the neck of the retort,
and

and the whole body of the receiver, should be covered with water, with a glass valve (such as Mr. Parker makes for his apparatus for impregnating water with fixed air) adapted to the upper part of the receiver, in order to let out, and collect, whatever air might be expelled or generated, but to admit of no return of air into the vessels; and when the process should be over to make an opening into the apparatus under water, which only would then be admitted to supply the place of the air. By making the process in these circumstances, carefully collecting all the air that should issue from the valve, measuring the quantity of water that should enter the vessels when the process was over, and comparing this quantity with the contents of the retort and receiver together, I could not fail to ascertain the fact in the most satisfactory manner possible. The particulars of the experiment were as follow, and they abundantly prove that air is not absorbed, but generated, in this process.

I put into the retort ten ounces of nitre, and eight ounces of oil of vitriol; and plunging the end of the retort, and the whole body of the receiver, in a trough of water, left an orifice, to which was fitted a glass valve. Towards the beginning of the process, twenty three ounce measures of common air were expelled; but very little afterwards, till near the end of the process, when I received thirty two

ounce measures of air, the bulk of which was so pure, that one measure of it required an equal measure of nitrous air to saturate it, and the very last that came was so pure, that it took twice its quantity of nitrous air, without being in the least increased in bulk.

Opening the receiver under water, there rushed into it twenty nine ounces of water, and I found that the whole cavity of the receiver and retort together, exclusive of the space occupied by the materials for the experiment, was forty six ounce measures; so that there were twenty six ounce measures of air generated. Had even the whole cavity of the retort and the receiver been filled with water on opening them, still there would have been nine ounce measures produced; for so much did the quantity of air expelled from the vessels exceed their utmost contents.

After this experiment, no reasonable doubt, I think, can remain with respect to the fact. For there cannot be any difference, in the nature of the thing, whether the distillation be made in a smaller, or in a larger way. However, I have since frequently distilled pretty large quantities of this acid; and though I did not make the experiment with so much attention as in the last case, every appearance that I did attend to confirmed me in the same sentiment. Nor do I see what could have given occasion to the
contrary

contrary opinion, besides the entrance of the air into the receiver when the apparatus was cooling, at the conclusion of the process, without considering how much had been expelled from the vessels while they were heating. It is possible, however, that if the process should not be urged to the last, the quantity of air produced might not supply the loss occasioned by the diminution of the common air, from the fumes of the generated spirit of nitre; since this diminution may be one fourth of the capacity of the vessels. But the loss must exceed this proportion before it can appear that there is any proper *absorption* of air in this process.

The source of the air produced in this process is, unquestionably, the acid of the nitre; when the heat is very great, I have observed that in this, as well as in other processes, a greater quantity of a powdery substance will be carried off by this acid more than it can hold in solution when it is cold. On one occasion in particular, when I was distilling a pretty large quantity of spirit of nitre, from nitre that was not very clean, and when the heat was accidentally greater than it ought to have been, a dense cloud of whitish matter poured from the retort into the receiver, quite filling the lower part of it, and rendering it almost intirely opaque; and the spirit of nitre which came over at that time, and which I received separately, in Mr. Woulfe's manner, had a

pretty large quantity of whitish sediment, exactly like that which I had sometimes collected after the production of dephlogisticated air that had been very cloudy. I have likewise had a sediment of the same kind in various processes for distilling spirit of nitre, though never so much of it as on this particular occasion. Something like it is also generally found after evaporating to dryness a quantity of spirit of nitre; in doing which I had, at one time, the following appearances, and with the account of them I shall close this section.

In evaporating a quantity of smoking spirit of nitre, in a deep phial, the fumes were first red, but afterwards quite transparent within the phial, and a small quantity of the white sediment remained, which became yellow when water was poured upon it.

There is also a small white residuum after the evaporation of oil of vitriol. This Dr. Withering found to be selenite.

SEC.

SECTION II.

Observations relating to the Colour and Strength of the Nitrous Acid, according to different Circumstances in the Process for making it.

IT is obvious to suppose that the more of the acid vapour is condensed in any given quantity of fluid, the stronger the acid must be; and it appears to me, that it is impossible to increase the strength of the acid, without in some measure heightening the colour of it, though the colour alone affords no sufficient indication of its strength. Because an addition of phlogiston, which in fact weakens the acid, by a kind of saturation, likewise heightens its colour; and before I made my own spirit of nitre, I was often deceived by this circumstance, and sometimes, I suspect, imposed upon; having bought spirit of nitre of a very weak quality, hardly distinguishable in colour from the very strongest. In general, the light yellow spirit of nitre is the best, containing with the most acid, the least phlogiston; but it seems to be impossible to procure an acid much exceeding the strength of this best common

fort, without giving it a deeper, or an orange colour.

The last part of every produce of spirit of nitre is of a deeper colour than the rest; and this I have always found to yield less nitrous air than the pale yellow acid that preceded it. And it is remarkable, that, though these acids be received in the same vessel, they will continue unmixed more than a day, and the uppermost may, with care, be poured off from the lower. When I first observed this, I thought it to be owing to the phlogiston making the spirit of nitre, which was highly charged with it, specifically lighter than the rest, and therefore disposed to remain at the top of it; till I observed the same thing concerning that phlogisticated spirit of nitre which always comes first, when substances containing phlogiston are mixed with the materials in the process. For this deep orange coloured spirit remains in the bottom of the phial, without seeming to be at all disposed to rise, and mix with the yellow spirit, which rests on the top of it.

I have frequently taken the produce of nitrous acid in the course of a distillation at several times, in order to make observations on their strength, and other phenomena attending the solution of metals in them. Of one of these processes I shall give a particular account, because I gave more attention to it than to any other, though I have frequently observed

served all the same things in various other distillations.

In distilling a large quantity of spirit of nitre I received the acid at four times. The first produce was very pale, and stronger than any that came afterwards. A quantity of it occupying the space of two pennyweights of water, produced, without heat, nine ounce measures and a half of nitrous air. It effervesced violently on mixing with water, emitting dense red fumes, and its action was the strongest at first. The second part of the produce was still paler, and yielded eight ounce measures and a half of nitrous air. This also mixed with water with effervescence, and its action was the strongest a little time after it was first applied. The third part of the produce was brown; it made no effervescence in mixing with water, and yielded seven ounce measures and a quarter of nitrous air, which came slowly at first, but quicker afterwards. The last part of the produce was of a deep orange colour; it made no effervescence in mixing with water, the air was produced equally, and was in quantity five ounce measures.

Repeating the experiments on the production of air with these acids the day following, I found that they all yielded considerably less, without heat, than they had done the day before; and even a boiling heat did not make them yield so much air as they had done before without heat. The difference also
in

in the production of air was in about the same proportion as the day before ; but the difference between the produce of air from the *last* produce of the acid, which yielded less air both *with* and *without* heat, was much greater than in the former, which yielded more air.

It was evident from this experiment, and the same was the result of several others, that the acid which comes over first in distillation is the strongest ; which may possibly be owing to its being the *purest*, in consequence of containing the least mixture of vitriolic acid : for it is made much less cloudy by the mixture of a solution of silver in the nitrous acid.

Thinking to procure a very strong spirit of nitre, I used oil of vitriol highly concentrated ; and I boiled the nitre which I used a long time in a glass vessel, so as to exclude all the *water* I could from the process, and admit as little *phlogiston* as possible. But though the produce was a spirit of nitre somewhat stronger than any that I had made before, the difference was not considerable ; nor could I be sure that, in a number of trials, the advantage would be on its side.

Though, in this process, I took all possible care to exclude phlogiston, the whole produce was of a brownish colour. On mixing the ingredients, a great heat was produced, and very red fumes presently

sently filled the retort ; whereas, in the common process, it is only a whitish cloud, like vapour of water, that rises first. On the application of heat, the retort presently became clear, and the red vapours passed into the adopter and receiver ; but towards the end of the process they re-appeared in the retort. Upon the whole, therefore, the phenomena of this distillation, except at the first mixing of the ingredients, did not at all differ from those of the common one.

That the brown, or deep orange colour, in the spirit of nitre, is imparted to it by phlogiston, evidently appeared by mixing a little charcoal with the other materials in this distillation ; when several adopters, which, foreseeing the consequence, I had purposely provided, were all filled with copious red fumes, and a very brown spirit of nitre was produced. Much air also was generated, part of which was fixed, and part strongly nitrous.

A small quantity of *brass dust* had a similar effect, in phlogisticating the spirit of nitre. The same also had a few drops of *spirit of wine*, and other substances containing phlogiston. But if much spirit of wine, oil of turpentine, or other fluid substances, of a similar nature, be made use of, the heat, and the quantity of vapour becomes excessive, and the process cannot go on.

When

When I used oil of vitriol that was only black, I found no sensible difference in the spirit of nitre, from that which had been got from oil of vitriol perfectly transparent; the quantity of phlogiston necessary to blacken oil of vitriol being too inconsiderable to have any sensible effect in this process.

I have observed that phlogiston deepens the colour of spirit of nitre, but it is remarkable that *heat* also, without any addition of phlogiston, produces the same effect; as may presently be observed by heating a quantity of the palest coloured spirit. This is a phenomenon exactly similar to that of the tubes and phials filled with the nitrous vapour, which also assume a deeper colour when they are made warm.

S E C.

SECTION III.

Of the Colour of the Nitrous Acid.

I HAVE observed that *heat* never fails to give a high orange colour to the palest spirit of nitre, and that with the less heat the acid is made, the lighter the colour of it will be. Having purposely made the process for distilling this acid with as little heat as possible, and taking care to have no phlogistic matter in the materials, I procured a large quantity of the acid (that which came in the middle of the distillation) as nearly as possible quite colourless, like water, and yet of the strongest sort.

I have also observed a farther, and a very remarkable change of colour in the phlogisticated nitrous acid, after being kept a long time in phials with good glass stoppers. For from being of the deepest orange, it has become quite *green*, the superincumbent vapour continuing still of an orange colour.

This change I first observed in a considerable quantity of nitrous acid, which had been of a light straw colour, and had assumed the deepest orange,

by exposure to heat in a glass tube hermetically sealed. This was also the case with several quantities of the acid incumbent on the crystals of oil of vitriol, of which I shall make frequent mention; and in one of the phials it had passed from green to a *deep blue*.

I must also take notice, in illustration of this fact, that, in the process for producing the nitrous vapour, viz. the rapid solution of bismuth, the liquid that comes over, mixed with the vapour, and which drops now and then from the end of the tube out of which the vapour issues, is generally of a deep blue.

Lastly, if a quantity of this deep green acid be put into a large phial, where the vapour has liberty to expand itself, it resumes its orange colour. This I have also observed is the case on pouring it on concentrated vitriolic acid.

I have since that made many more observations relating to the colour of this acid; and I think I have decisively proved, that neither this acid, nor the muriatic, have, naturally, any more colour than the vitriolic acid, or than water itself; being able to give them colour, change it, or wholly take it away at pleasure; and some of the circumstances in which these changes take place are not a little remarkable.

The

The facts that I shall relate prove that it is either *phlogiston*, or *mere heat*, that gives colour to this acid, that this colour may also be all expelled by heat; but that continuance of heat will give it more colour, and deepen it at pleasure, so that more heat, in glass vessels hermetically sealed, seems to have the same effect with *phlogiston*. But, more probably, heat affects it in such a manner, as to develope, as it were, the *phlogiston* it contained before, and put it into a *new state*, rendering that part of the acid to which it is attached both more volatile, and also disposed to reflect the rays of light in a particular manner; whereas, before this action of the heat, the *phlogiston* was *latent*, at least, did not evidence itself by those particular effects. It will appear, however, hereafter, that heat gives colour to the nitrous acid by expelling the pure air, which leaves the rest *phlogisticated*.

On the first of August, 1777, I resumed my experiments on this subject; when, having provided a sand furnace, to be kept hot for a considerable time, for many purposes that will be mentioned in the course of this volume, I put a quantity of strong and pale coloured spirit of nitre into a glass tube, about an inch in diameter, and three feet long; and, sealing it hermetically, I placed it in the warm sand. Taking it out after some time, I found it orange coloured; and though it was more deeply

coloured while it continued hot than it was afterwards, it retained so much of the colour, as to be ever after of as deep an orange colour as spirit of nitre is generally found to be. And though before this process the vapour rising from it was quite colourless, there being nothing visible above the surface of the acid, in the phial from which it was taken, the whole tube (which I have observed was three feet in length) was uniformly filled with the dark orange coloured vapour.

This process being performed in a glass tube hermetically sealed, I was fully satisfied, that this colour which the acid had assumed could not be owing to any thing besides heat. That it was not owing to any thing peculiar to the glass of lead, of which, in a great measure, flint glass consists, was evident from observing the same effect on the acid when the experiment was made in common green, or bottle glass.

Having about the same time exposed to a heat of some continuance several quantities of blue and green spirit of nitre, it may not be improper to note the results of these experiments in this place. In one instance, the green spirit of nitre became orange coloured; but when it was cold it was almost as green as at first, though there was evidently a mixture of yellow in it.

When

When I had exposed a quantity of blue spirit of nitre in a long glass tube a few days, the blue colour was barely perceivable. It was placed in the sand furnace on the 23d of August, and on the 30th of September following it was entirely colourless, and had no visible red vapour over it when cold.

I also exposed to a very moderate heat a small phial with a ground stopper, almost filled with a deep blue nitrous acid, when it presently assumed a deep green, and when it was cold it resumed its former blue colour. In this experiment the heat had not been continued sufficiently long to produce a permanent change of colour. For, having exposed to a moderate heat, in a long glass tube, hermetically sealed, a quantity of blue nitrous acid, it lost its blue colour, and assumed a yellow one; and when it was cold the blue colour did not return, except in the smallest degree.

I did not, however, come to this conclusion concerning the cause of the change of colour in this acid in the summary manner above described; but in consequence of a series of observations, attended with a variety of circumstances, some of which were remarkable enough.

A little time before I had made the experiments above recited, I had begun a new mode of examining a variety of fluid substances; which was to

put a small quantity of the fluid into a glass tube, three or four feet long, and sealing it hermetically, to expose the end containing the fluid to as great a degree of heat as I found it could bear; and to keep it in that heat a considerable time. My design in providing tubes of this length was to give room enough for the vapour to expand, and condense in the remote and cool end of the tube, while it was boiling in the other end.

In this manner I exposed to the influence of heat a small quantity of spirit of nitre, as I had done a variety of other fluid substances, without any particular expectation.

The acid, however, no sooner felt the heat than it exhibited appearances that engaged my attention very strongly.

The spirit of nitre I made use of was of the strongest and palest sort, without the least perceivable red vapour over the surface of it. The glass tube in which it was confined was about four feet long, and about one third of an inch in diameter, and the space occupied by the acid was two inches in length. The tube thus prepared I held in my hand, presenting the end in which was the spirit of nitre to a common fire, and holding the tube in an inclined position. The first effect of the heat to which it was exposed, was its assuming an orange colour throughout. After this, a deep
I orange

orange coloured vapour, appeared above the surface of the acid, and gradually ascended higher into the tube, at the same time that the acid itself grew paler, and at length became quite colourless, like water: all the colouring matter being, to appearance, driven out of it.

This red vapour kept rising higher and higher in the tube, leaving a considerable space, some times of ten or twelve inches, between it and the acid, all which space was quite transparent. This was a very pleasing appearance, and it was amusing to observe the space occupied by the red vapour, which extended three or four inches, every thing else in the tube above and below it being transparent, and the red spot itself receding from the acid as the heat increased, or approaching to it as the heat diminished.

I observed, however, that by the continued application of heat the quantity of red vapour increased, and the colour grew manifestly deeper. I then withdrew it from the fire, and presently saw the red vapour descend lower and lower, till it reached the colourless acid at the bottom of the tube, and, entering into it, communicated to it its own orange colour. But when it was quite cold, I did not, at that time, perceive that the acid was of a deeper colour than it had been at the commencement of the process, and no visible vapour remained upon

it. To produce a permanent colour, as I observed before, more *time* was requisite.

When one of these tubes had been thoroughly heated two or three times, and the last time had been exposed to a boiling heat for about an hour (the heat having been such as to keep the acid quite colourless, and likewise to make a large colourless space above the acid) I let it cool in a very good light, and then observed, that as the red vapours descended, and the condensed liquor, highly charged with it, trickled down the tube, and mixed with the colourless acid below, it made *waves* in the acid, something like oil in water, or rather like the mixture of a strong acid in water, and that this denser acid descended in these visible waves to the very bottom of the liquor; and yet when the depth of the acid was about two inches, the upper part was sensibly darker coloured by this means than the lower. I also observed, that while the acid was acquiring its colour, as long as it continued tolerably warm, a vapour kept issuing out of it, and dancing in a beautiful manner to the height of an inch, or two inches, above the surface of it.

I had several tubes in which this process had been performed, one of which was an inch wide, and three feet long; and though it had only a small quantity of acid in it, originally of a pale colour,
and

and without any visible vapour, the whole of that large tube was filled with the densest orange coloured vapour expelled in this manner from the pale acid, and it continued so more than a year, without any appearance of the vapour entering into the acid again; except that the colour of the acid, from being of a deep orange, which it retained a considerable time, became quite green. This was also the case with a pretty large quantity of the acid, which had been quite pale, but was made of a deep orange, by exposure to heat in glass vessels hermetically sealed, and in that state transferred into a phial with a ground stopper, and which was kept close shut near a year.

I had now tubes filled with the red vapour of spirit of nitre exactly resembling those of which an account will be given hereafter, made by the rapid solution of bismuth in spirit of nitre; and I found that these had the very same property. For whatever part of these tubes I heated with the flame of a candle, it became of an intensely orange, or red colour, while the parts both above and below it, which were not heated, remained unchanged.

Having been much pleased with this expulsion of all the colouring matter from a quantity of spirit of nitre; and seeing it in the form of vapour confined to the space of four or five inches, in the middle of a very long glass tube, which was quite

transparent above and below it, I made several attempts to separate this coloured vapour from the fluid, out of which it had been expelled, by melting the tube in the intermediate colourless space, and sealing it hermetically. But these attempts were in vain, on account of the increased expansive force of the vapour in that heated state. The air was expelled from the *acid*, which I was not then aware of.

I imagined that I might, by this means, when the tube was quite filled with red vapour, and cold, take it off from the acid, and preserve it red and dry, or nearly so. But in attempting this, I presently found that there had been a great increase of elastic matter within the tube. For the moment I had a little softened a part of the tube, in order to take it off from the rest, the red vapour rushed out with great violence. It is possible, however, by this means, to get a tube filled with a moderately red vapour. But soon after I hit upon a much easier method of effecting the same thing.

Though I could not separate the red vapour from the colourless acid while it was boiling, it was very easy, I found, by boiling the acid in a short tube, or phial, to expel all the colouring matter from it, and thus to get a quantity of spirit of nitre quite free from all colour; which I accordingly did, and then imagined that, the coloured
vapour

vapour being wholly expelled from it, the acid would always continue colourless. And so, indeed, it did after it was quite cold; and it will continue without return of colour, and be but little diminished in quantity, or impaired in strength, so long as it is kept from the contact of any thing that contains phlogiston, or from much heat. But, to my great surprise, at that time, I found that either of those circumstances would make this colourless acid resume its former colour, or acquire a deeper one than it had before. It was, however, by accident, that I first learned this.

Having procured a quantity of nitrous acid quite colourless, I put a part of it into a phial which had a common cork (a phial with a glass stopper happening not to be at hand) and not suspecting that this circumstance would affect the colour of the acid, which was a considerable distance from the cork. I found, however, after two days, when I took out the cork, that the acid smoked very much, and had completely recovered its original yellow colour, so as not to be distinguished at sight from what it had been before the colouring matter had been expelled from it. I then took a part of this acid, and inclosing it in a glass tube, which I sealed hermetically, exposed it to the heat as before, when it became of an orange colour; and resuming the process in an open tube, I drove out the
the

the colouring vapour once more, and made the acid a second time transparent.

I found, however that a little phlogistic matter has a quicker and more remarkable effect on this colourless acid than mere heat. I put a part of the colourless acid into one of the tubes above-mentioned, and kept it boiling a whole day before the fire, and the night following in a sand heat, without being able to perceive any sensible change in it, though a slight redness was apparent on the first application of the heat. But having put another part of the same original quantity of the colourless acid (which from the preceding experiment will be judged to have been very weak) into a phial with a common cork, at the distance of an inch from it, I observed that in a few hours only, the upper part of the acid was become yellow, and the next morning it was yellow throughout, exactly like the best nitrous acid when fresh made.

But no instance of a change of colour in this acid by heat was so very remarkable as the following. Having put a small quantity of pale colourless acid, into a short glass tube, and almost burying it in the hot sand, I found the next morning, that the whole tube was quite filled with red vapour, and the acid itself was quite red, and perfectly opaque, and to appearance a little *viscid*, like red ink. Neither before, nor since, have I ever seen

seen nitrous acid in that state. It even retained the same appearance which was not orange, but a proper and a very deep *red*. Being quite cold, I could examine it at my leisure. It was the only appearance I ever had of the kind.

Replacing the same tube in the sand heat, and taking it out some time after, the acid was of a deep orange while hot, but not very deep, and rather of a pale colour when cold; but there was a little whitish matter formed on different parts of the glass, of which a farther account will be given presently.

I soon found that the *close confinement* of the vapour contributed greatly to this change in the acid. A quantity of colourless acid being put into a short thick tube hermetically sealed, and placed in the sand heat, in about an hour had red fumes, and in an hour more the acid was orange coloured. Whereas a quantity of the same acid confined in a *long tube* the same time, and in the same degree of heat, had acquired red fumes only, while the acid itself remained colourless.

In all the circumstances in which much heat is given to spirit of nitre, it necessarily acquires a deeper colour. This is the reason why, in all my attempts to procure a very strong spirit of nitre, by using concentrated vitriolic acid, and boiling the nitre, in order to expel the water it contained,
it

it was always of an orange colour. For, in this case, the mixture of the oil of vitriol and nitre was attended with great heat.

I believe that any degree of heat, sufficient to throw the acid into the form of vapour, will always give it more colour than it had before. This I found to be the case when I re-distilled a quantity of spirit of nitre from fresh nitre, in order to purify it from any vitriolic acid that might remain in it. The result of this process was an acid of a deeper colour, and that smoked more than it did before.

Heat is not necessary to make spirit of nitre colourless. For exposure to the open air does the same thing, and probably with less dissipation of the acid. During this exposure to the open air, the nitrous acid, if it be strong, increases considerably in bulk and weight, in which it resembles the vitriolic acid, though this is not in the smallest degree volatile. In order to observe more distinctly the whole of this process, some time in the month of July, 1777, I exposed to the open air, in a common glass tumbler, about three ounces of orange coloured smoking spirit of nitre. In a day or two it was quite colourless, but a fly, or any small substance containing phlogiston, falling into it, would colour the surface of it again for a considerable time, though at length these accidents had less effect

fect upon it. This acid kept increasing in bulk to the April following, when the quantity was considerably more than doubled; but from that time it began to decrease, and continued so to do till more than half that it had gained was gone, after which it continued very much the same for several months.

The circumstances relating to the *white matter*, which I have observed was formed by the nitrous acid in glass tubes hermetically sealed, and exposed to a continued heat, I am not able to explain. I first observed it in that short tube in which the phenomena of the colour of the acid were so very remarkable, and indeed singular; but afterwards it never failed to make its appearance whenever the acid had been long confined, and exposed to much heat, but the quantity procured was too inconsiderable to make many experiments upon it.

It was on the 25th of September that I observed this white, or yellowish, matter in the tube above-mentioned. On the 30th of the same month, I observed that the colour of the acid was rather lighter, and beside that whitish matter at the bottom of the tube, there was a similar concretion adhering to the sides of the glass, just above the surface of the acid, the colour of which was partly yellow, and partly green.

Having

Having got more of this white matter in other tubes, I observed that it was easily scraped off from the glass, and left it transparent, so that it seems to be something deposited from the acid, and not an abrasion of the glass. It was not at all affected by distilled water, but spirit of salt dissolved it entirely, and became of a yellow colour inclining to orange. Applying the flame of a candle to that part of the glass tube on which some of this white matter lay, it was dissolved, and dispersed in *white*, not *red* vapours. An earthy pellicle remained, not easily affected by heat, but it was dispersed when it was made red hot with a blow pipe. This pellicle adhered firmly to the glass, but in time it was completely dissolved by spirit of salt, which assumed the colour above-mentioned.

It is pretty evident, from this observation, that this matter did not really contain spirit of nitre *as such*. For had it contained the proper nitrous acid combined with any earthy matter, as the calx of the lead in the glass, the spirit of salt could not, I apprehend, have decomposed it. In other respects it had very much the appearance of minium become white by imbibing nitrous vapour. But this is not at all affected by spirit of salt.

It was evident, however, that wherever this white matter was formed, the quantity of the acid

was

was diminished, so that it looks as if the acid itself was destroyed, and converted into something of a different nature.

SECTION IV.

On the Phlogification of Spirit of Nitre.

IN the preceding experiments, I found that the colourless acid became smoking, or orange coloured, and emitted orange coloured vapours, on being exposed to heat in long glass tubes, hermetically sealed; and I then concluded, that this effect was produced by the action of *heat*, evolving, as it were, the phlogiston previously contained in the acid. Afterwards, having found that it was not *heat*, but *light* only, that was capable of giving colour to spirit of nitre, contained in phials with ground stoppers, in the course of several days; and that in this case the effect was produced by the action of *light* upon the *vapour*, which gradually imparted its colour to the liquor on which it was incumbent, I was led to suspect, that as the glass

tubes, in which I had formerly exposed this acid to the action of heat, were only held near to a fire, in the day-light, or candle-light, it might have been this *light*, which, in these circumstances, had, at least in part, contributed to produce the effect.

In order to ascertain whether the light had had any influence in this case, I now put the colourless spirit of nitre into long glass tubes, like those which I had used before, and also sealed them hermetically, as I had done the others; but, instead of exposing them to heat in the open air, from which light could not be excluded, I now shut them up in gun barrels, closed with metal screws, so that it was impossible for any particle of light to have access to them; and I then placed one end of the barrels so near to a fire as was sufficient to make the liquor contained in the tube to boil, which I could easily distinguish by the sound which it yielded. The consequence was, that in a short time the acid became as highly coloured as ever it had been when exposed to heat without the gun barrel. It was evident, therefore, that it had been mere *heat*, and not *light*, which had been the means of giving this colour to the acid, and which has been usually termed *phlogisticating* it.

When I made the former experiments, I had no suspicion that the *air* contained in the tube had any concern in the result of them; and, in those which
I made

I made in the phials in a moderate heat, I found that the acid received its colour when the best *vacuum* that I could make with an air pump was over it.

My friend Mr. Kirwan, however, having always suspected, that the *air* was a principal agent in the business, I at this time gave particular attention to this circumstance; supposing that, if any part of the common air had been imbibed, it must have been the *phlogisticated*, and that it was the phlogiston from this kind of air which had phlogisticated the acid. The real result, however, was not so much in favour of this supposition as I had expected; for the principal effect of the process was the emission of dephlogisticated air, so that the acid seems to become what we call phlogisticated, by parting with this ingredient in its composition.

I put a small quantity of the colourless acid into a long glass tube, which besides the acid would have contained 1.23 ounce measures of common air, but that the vapour of the acid excluded about one twentieth of the quantity. Having sealed the tube hermetically, I shut it up in a gun barrel, in the manner mentioned above, and exposed it to a boiling heat for several hours, and then opening it under water, there came out of it 2.03 ounce measures of air, very turbid and white; and when it was examined, it appeared to be of the standard

of 1.02, with two equal measures of nitrous air; when with one measure of the same nitrous air the standard of the common air was 1.07. The quantity of phlogistified air absorbed in this experiment I ascertained by the following computation.

As one measure of common air, and an equal quantity of nitrous air were reduced to 1.07 m. it is evident, that 0.93 m. had disappeared; but as this was effected by the nitrous air uniting with all the dephlogistified air contained in the common mass, and as they unite in the proportion of one measure of dephlogistified air to two measures of nitrous air, one third of the 0.93 m. *viz.* 0.31 m. will be the quantity of dephlogistified air that was contained in the one measure of common air on which the experiment was made, the remainder, *viz.* 0.69, having been phlogistified air. The common air contained in the tube would have been 1.23 oz. m.; but deducting from it one twentieth in the whole, it will only be 1.17 oz. m. I then say, if one measure of this air contains 0.69 m. of phlogistified air, 1.17 oz. m. will contain 0.8073 oz. m. of phlogistified air. This, therefore, was the quantity of phlogistified air which had been exposed to the action of the acid of nitre in the tube.

In order to find how much of the same kind of air was contained in the tube *after* this process, I examined

examined the result above mentioned in the following manner. Since two measures of nitrous air, and one of this residuum, were reduced to 1.02 m. it is evident, that 1.98 m. had disappeared, and consequently one third of this quantity, viz. 0.66 m. had been dephlogisticated air, and that the remainder of the measure, viz. 0.34, had been the proportion of phlogisticated air in one measure of this residuum. If then one measure of this residuum contains 0.34 m. of phlogisticated air, 2.03 oz. m. will contain 0.6902 oz. m. which is less than 0.8073 oz. m. the quantity contained in it before the process: so that a part of the phlogisticated air had been either absorbed or decomposed, its phlogiston having been imbibed by the acid at the same time that it had emitted the dephlogisticated air.

In another process of the same kind, the glass tube contained 0.92 oz. m. of common air, and the air that came out of it after the process was one ounce measure, of the standard of 1.6, with two measures of nitrous air; and computing as I did before, the phlogisticated air in the tube before the process was 0.6072 oz. m. and after the process 0.54 oz. m.

In these computations it is supposed, that the air emitted by the acid was perfectly pure, so that all the phlogisticated air that is found after the process is supposed to have been contained in the common air confined in the tube before it was commenced.

But I found, that the air emitted by the acid is by no means perfectly pure, so that much of the impurity must be ascribed to this circumstance.

In order to exclude all air from the contact of the acid, I made a quantity of it to boil in the tube, and when the vapour had expelled all the air, I sealed it hermetically, in the manner in which water hammers are made; and then exposing it to heat, found that it acquired as high a colour as when air had been confined along with it; so that it is evident, that *air* is not necessary to this effect. When the tube was opened under water, a quantity of dephlogisticated air rushed out, exceedingly white as before; but when I examined it, I found it to be of the standard of only 0.66. When this impurity is considered, it will appear, that when much air is yielded in this process, some phlogisticated air may have been imbibed, though, computing in the manner above mentioned, the phlogisticated air after the process should be in greater quantity than was contained in the tube before it, as was the case in the following experiment.

In a glass tube which, besides the acid, contained 1.13 oz. m. of common air, I exposed colourless spirit of nitre to heat till it became of a deep orange colour; and when it was opened under water, there came out of it 2.83 oz. m. of air exceedingly turbid, of the standard of 0.66, with two equal quantities of

nitrous air, when that of the common air, with one equal quantity of nitrous air, was 1.07. Computing in the manner above mentioned, there was in the tube before the process 0.7477 oz. m. of phlogisticated air, and after the process 0.8792 oz. m. But the dephlogisticated air, amounting to 1.7 oz. m. being of the standard of 0.66, will be found to contain 0.374 oz. m. of phlogisticated air, which being deducted from 0.8792, there will remain only 0.5052 oz. m. which is considerably less than 0.7477 oz. m.

That the nitrous acid can become coloured, without imbibing any thing from phlogisticated air, is evident not only from its becoming so when heated *in vacuo*, as described above, but also, when it was in contact with any other kind of air, as free from phlogisticated air as I could make it. But from the manner in which these experiments were necessarily made, it was impossible intirely to exclude phlogisticated air, either as part of the atmospheric air, or as contained in the impurities of the air that I made use of; for I first filled the tube with spirit of nitre, then plunging the orifice of it in a vessel of the same, I introduced a quantity of the air which I wished to expose to it. After this, putting my finger upon the orifice, I turned it upside down, and applying to it the closed end of a glass tube, of about the same diameter, I sealed it hermetically with a blow-

pipe as expeditiously as I could. This is a necessary imperfection in the experiment ; but I know not how to remedy it, if any of the acid is to be left in the tube. However, the phlogisticated air introduced in this manner from the atmosphere, must have borne a very small proportion to the air in the tube ; and some objection will always remain to the experiment from the impurity of the dephlogisticated air made use of.

Having repeatedly observed that the acid became coloured in consequence of being exposed to heat in contact with any kind of air whatever, I exposed at the same time, and in the same circumstances, three equal quantities of the same colourless spirit of nitre, in three nearly equal tubes, one containing dephlogisticated, another phlogisticated, and a third inflammable air ; that, if there should be any difference in the colouring of the acid in these cases, it might be the more easily perceived. But though I gave all the attention that I could, I did not perceive that there was any difference, except what arose from some of the tubes being placed a little nearer the fire than the rest ; and, by changing their places, the colour was at length the very same in them all.

As in these three cases I examined the air before and after the process, in the manner above mentioned, I shall just recite the particulars.

Of

Of the dephlogisticated air the tube contained before the process 1.46 oz. m. of the standard of 0.67, and after the process it contained 1.76 oz. m. of the standard of 0.77; a difference owing in part to the mixture of common air, which could not be excluded in the sealing of the tube, and in part to the air emitted from the acid not being pure.

Of the phlogisticated air, the tube contained 1.3 oz. m. and after the process 1.95 oz. m. of the standard of 1.38.

Of the inflammable air, the tube contained before the process 1.52 oz. m. and after the process 1.9 oz. m. of the standard of 1.8. They were all measured by a mixture of two equal quantities of nitrous air.

If these results be examined as that of the first experiment, with common air, it will be found that, in all these processes, there was less phlogisticated air, or inflammable air, after the process than before; and this result being thus uniform, I cannot help concluding, that this kind of air is in part decomposed, and purified by this means; so that by this emission of dephlogisticated air which the heat expels from the acid, something, and probably phlogiston, is at the same time imbibed from it; which proves that phlogisticated air is no simple substance, but a compound, and that phlogiston is one constituent part of it; for this acid acquires the

same colour, and all the same properties, by adding to it any thing that is supposed to contain phlogiston.

As the spirit of nitre can be rendered smoking, or phlogisticated, by the mere expulsion of dephlogisticated air, it is evident, that it contains two principles in close affinity with each other, and that nothing is necessary to render either of them conspicuous besides the absence of the other.

It is also natural to suppose, that, for the same reason that the *dephlogisticating* principle (as it may be called) is expelled, the *phlogisticating* principle should enter; so that the purification of the air in contact with the acid may be a necessary consequence of the expulsion of the pure air contained in it, the whole tending, as it were, to an equilibrium in this respect. It is therefore by no means difficult to conceive, that phlogiston should be extracted from the contiguous air at the same time that the dephlogisticated air *not pure* (that is, containing a mixture of phlogisticated air) is driven out of it; for the acid always containing phlogiston, whatever air is contained in it, and expelled from it, may necessarily contain phlogiston or phlogisticated air; but the purer air may be emitted, and the less pure air be imbibed, till the whole come to be of the same quality. It may, however, perhaps follow from the emission of impure dephlogisticated air, and the imbibing

bining of phlogisticated air at the same time, that the former does not consist of dephlogisticated and phlogisticated air loosely mixed, but of some intimate union of dephlogisticated air with phlogiston, though they may be separated by a mixture of nitrous air, and other processes, in the very same manner as dephlogisticated air may be separated from a loose mixture of phlogisticated air.

It is evident from these experiments, that a red heat is not necessary to the conversion of nitrous acid into pure air, though this process, as appeared by my former experiments, produces this effect most quickly and effectually.

I cannot help considering the experiments above recited to be favourable to the doctrine of phlogiston, and unfavourable to that of the decomposition of water, though not decisively so. For since the red vapour of spirit of nitre unquestionably contains the same principle that has been termed phlogiston, or the principal element in the constitution of inflammable air, and according to the antiphlogistians, this is one constituent part of water, they must suppose, that the water in this acid is decomposed by a much more moderate heat than in most other cases. In general, I believe, they have thought a red heat to be necessary for this purpose. It is evident, that the conversion of water into steam by boiling,

boiling, or by any heat that can be given to it under the strongest pressure, has no tendency whatever to decompose it. But if the mere boiling of water in nitrous acid could produce this effect, I do not see why the same should not be the case when water alone is boiled.

I think it will also be more difficult to explain the purification of the incumbent atmospherical air on the antiphlogistic than on the phlogistic hypothesis, whatever be the constitution of phlogisticated air.

As, in the experiments above mentioned, *heat* without *light* gives colour to the nitrous acid, and the reflection or refraction of light is always attended with heat, it may perhaps be *heat* universally that is the means of imparting this colour, though the mode of its operation be at present unknown. And in these experiments, as well as the former, it is the *vapour* that first receives the colour, and imparts it to the liquid when it is sufficiently cold to receive it.

The rushing out of a quantity of turbid white air from a transparent tube, quite cold, is a striking phenomenon in these experiments. It may be worth while to examine of what it is that this remarkable cloudiness of the air consists. There is the same appearance, as I have more than once observed,

observed, in the rapid production of any kind of air, which is perfectly transparent as it passes along the glass tube through which it is transmitted, till it comes into contact with the water in which it is received.

N. B. The mixture of nitrous air with common air, mentioned in this section, was made with agitation.

SECTION V.

Of the Composition of Spirit of Nitre from dephlogisticated and inflammable Air.

THAT water consists of two kinds of air, dephlogisticated and inflammable, is now, I believe, generally admitted as one of the most important, and best ascertained, doctrines in chemistry. My own experiments having seemed to favour it, I made no difficulty of receiving it myself; but having, at the time of the publication of the last of the six volumes of my experiments, found that, in decomposing the two kinds of air above mentioned by the electric spark, I got much less water than I expected, and, instead of it, a dark coloured vapour, not easily condensed,

I could

I could not help concluding that something yet remained to be investigated with respect to this subject, and determined, at a proper opportunity, to resume my inquiries into it.

At that time, however, I had no suspicion of any *acid* being produced in the process; having never been able to find any in the water which I had hitherto procured in pretty large quantities from the decomposition of those two kinds of air, though the doctrine of dephlogisticated air being, or containing, the principle of *universal acidity*, had been advanced by M. Lavoisier, and admitted by myself and others.

Suspecting that much of the water which had been procured in the above mentioned process was no proper constituent part of the air, but only such as had been diffused through it, and in some manner attached to it, and kept suspended in it, and therefore might be separated from it, without decomposing the air; on resuming these experiments, I used every precaution I could think of to detach all water from the air on which I operated. In order to this, I kept it confined by mercury, together with a quantity of *fixed ammoniac*, which imbibes water more readily, if not in greater quantity, than quick lime, or any other known substance.

In this more accurate method of making the experiment, I was gradually led to discover the acid,
which

which had escaped my observation before. But I am not certain that I should have found it even now, if I had not been aided by the sagacity of Mr. Keir, who was always of opinion, that some acid *must* be the produce of this experiment, or rather that the produce would be something which would become acid by exposure to the open air.

I began with making the explosions in the same glass vessel from which the mixture of air had displaced the mercury with which it had been filled; when I found, as I have observed in my last publication, the whole of the vessel was filled with a dense smoke, which settled into a black coating of all the inside of the vessel, and which appeared, as before, to be mercury; becoming white by exposure to the air. For some time I perceived no appearance of *water*; but placing the vessel at a proper distance from a fire, I found about a quarter of a grain collected on the opposite side; when, as the vessel contained four ounce measures of air, the water produced ought to have been at least a grain.

The mercury being an impediment in this process, I afterwards confined the mixture of air in one vessel (with mercury and fixed ammoniac as before) but I made the explosions in another, which I had previously exhausted of air. This vessel was larger than that which I had used before, containing something more than eight ounce measures; so that the
air

air it contained, being one third dephlogisticated and two thirds inflammable, would have weighed about two grains. After one explosion the quantity of water collected appearing inconsiderable, I repeated the process in the same vessel, and then collecting the water, I found it not to exceed a grain and a half.

I repeated this experiment very often, and constantly found some water, but it always fell far short of the weight of the air decomposed. There must, therefore, have been something not very fluid adhering to the sides of the vessel, which could not be dislodged by a moderate heat; and indeed the glass did not recover the perfect clearness that it had before the process.

I always observed, that, presently after every explosion, the vessel was filled with a dense vapour, so that it was sometimes impossible to see through it; and before I admitted the external air, I could pour it from one end of the vessel to the other, and it seemed to fall almost as fast as a feather in a common vacuum, and in general it did not disappear in less than ten minutes. I even found this dense vapour when the mixture of air had been confined by water. The smell of the vessel, after the process, was that of the most offensive kind of inflammable air from iron.

From

From these experiments it was sufficiently evident, that something more than *water* had been produced; and pouring into the vessel a quantity of the juice of litmus, it was instantly turned to a deep red; so that it was equally evident, that an *acid* had been formed. In all the preceding experiments the dephlogisticated air had been procured from manganese; and in all the experiments mentioned in this section, the inflammable air was from iron by water only.

A great number of strong glass vessels having been broken in these experiments, and sometimes with some hazard to myself, and the quantity of air that I was able to decompose in them being small, I next procured a *copper* vessel, which contained about thirty six ounce measures of air; and having now no other object than discovering the *kind* of acid that I had procured, I made repeated experiments in it; and after every ten or twelve explosions collected all the liquid matter I could find; which, as the air had been previously confined by water, was pretty considerable, about equal to the weight of the air.

The liquor that I procured in this manner was always of a deep blue or green, being evidently a solution of copper. But it also contained a redundant acid, as appeared by its turning the juice of litmus red. Besides this blue liquor, there was
always

always a quantity of seemingly abraded copper ; for it was perfectly and quickly dissolved by volatile alkali, as copper very minutely divided would have been.

In these experiments I used, at different times, dephlogisticated air from manganese, from red precipitate, and from red lead, as the most unexceptionable of all ; and as it was obligingly furnished me by Mr. Keir, the preparation of it may be depended upon. There did not, however, appear to be any other difference in the liquors produced by means of these kinds of dephlogisticated air, except in the shade of the colour ; that from manganese being of the deepest blue, and that from red lead the lightest ; and this difference might be accidental.

By the assistance of Mr. Keir I examined these solutions of copper, and presently found, by means of a solution of terra ponderosa in spirit of salt, that it was not, in any of the cases, the vitriolic ; and yet, as the dry substance left by the evaporation of the liquor did not deliquesce, I had concluded, that the acid was neither the nitrous, nor the marine ; but Mr. Keir informs me, that this is the case with a fully saturated solution of copper in spirit of nitre.

Also Dr. Withering, who was so obliging as to examine some of these liquors for me (for, not being much accustomed to these analyses, I had requested him to undertake it) had procured from that in the
production

production of which the *red lead* had been used, crystals of nitre, and other indisputable indications of nitrous acid; so that I was satisfied that it was this acid that was produced in all the cases.

I had a farther proof of the acid being the nitrous, that having (in order to get a quantity of liquor that should be as little saturated with any metal as possible) used a vessel of *tinned iron*, I found, that after some time, when the tin had been much corroded (and with every process a considerable quantity came away) the liquor, which at first was colourless, was tinged with red. In these experiments I made use of dephlogisticated air from red lead.

As both the kinds of air made use of in these experiments were exceedingly pure, it seems evident, that phlogisticated air does not contain all the elements of nitrous acid; but only supplies a *base* for it, the dephlogisticated air (which was used in a greater proportion in the valuable experiment of Mr. Cavendish) supplying the acidifying principle, as I had conjectured. Besides; though all phlogisticated air could not be excluded in those experiments in which the air pump was used, this objection cannot well be made to those in which that instrument was not used; and in them the slowly condensable vapour above-mentioned seems to be an evident symptom that the produce was

not mere water. But it is a satisfactory answer to this objection, from the presence of *phlogisticated air* in the tube, that this kind of air is not decomposed, or at all affected, by this process, as will be found by mixing any quantity of it with the two other kinds of air.

That a considerable quantity of water enters into the composition of dephlogisticated air, will not be thought improbable, when it is considered that, in my former experiments, this appeared to be the case with respect to *inflammable air*. For without water this air cannot be procured. I can also now say, that the same is the case with respect to *fixed air*. It is not therefore improbable, that the same may be true of every other kind of air, since water is used in the production of them all.

When I wrote the preceding, I had found that the decomposition of dephlogisticated and inflammable air, by means of the electric spark, produced an *acid liquor*, which Dr. Withering found to be the *nitrous*; though I should have observed, that he expressed some doubt whether the liquor did not also contain some other acid besides the *nitrous*.

I have since that time been desirous to ascertain the quantity of *acid* producible from a given quantity of air; and with this view I gave Mr. Keir as much of the liquor as I had collected from the decom-

decomposition of about five hundred ounce measures of dephlogisticated air, and the usual proportion of inflammable air mixed with it. The liquor, he informed me, was 442 grains, of the specific gravity of 1022 (that of water being 1000) and that it contained as much acid as was equivalent to 12.54 grains of concentrated acid of vitriol; which quantity of vitriolic acid is capable of saturating as much vegetable fixed alkali as is contained in twenty two grains and a half of dry nitre, or about twenty three grains and a quarter of nitre crystallized in mean temperature. The sediment of the same liquor he also supposed to contain, at least, as much acid as the liquor itself.

That this sediment contains much acid, seems evident from this circumstance, that, when it is first formed, it often emits small bubbles, which rise to the surface of the liquor, and continue to do so a considerable time. This was more particularly the case with the sediment which I had from the tinned iron tubes. These small bubbles, I imagine, consist of nitrous air (formed from the superabundant acid vapour adhering to the metal and the water in the liquor) because when a phial, half filled with this liquor, had stood about a week, the air on the surface of it instantly, and repeatedly, extinguished a piece of lighted wood that was dipped into it.

From the preceding *data*, given me by Mr. Keir (and making allowance for the indefinite quantity of water contained in the concentrated acid of vitriol) I am inclined to think, that not much more than one twentieth part of dephlogisticated air is the acidifying principle, and that nineteen parts are water.

This, I would however observe, relates to air fully saturated with water, in consequence of its having been kept in jars standing in water, so that I think it possible that the water in the driest dephlogisticated air may not amount to more than nine tenths of its weight. But I have not ascertained, by any experiment, how much water any of the kinds of air are capable of holding in a diffused state, without being any necessary part of their constitution*.

Though Mr. Keir found the greatest part of the acid in the liquor with which I furnished him to

* Reflecting farther on the subject, and especially on the quantity of phlogisticated acid vapour that escapes in all these processes, I think there is reason to conclude, that there is much more of the acidifying principle, and less of water, in the composition of dephlogisticated air than is here supposed. By making the experiment with more care, and especially allowing the tube more time to cool after each explosion in it, I have actually found more acid than I had when this article was first written. Still, however, I should imagine, that without using particular care to dry the air, the acid may be only one tenth of the weight.

be the *nitrous*, there were evident signs of its containing a small portion of *marine acid*, by its making a precipitation with a solution of silver in nitrous acid. But this mixture of marine acid, he observes, is constantly found to accompany the production of nitre in the operations of nature. Whether the different substances from which the dephlogisticated air was extracted made any difference in this case, I cannot tell; but that which I gave Dr. Withering was from minium, and that which Mr. Keir examined was from manganese.

In the notes which I took of the first production of this liquor I termed it *blue*, and Dr. Withering also calls it blue, and once a *greenish blue*; but that which I gave Mr. Keir, and all that I have got since, is a decided and deep *green*, which Mr. Keir thinks to be owing to the phlogistication of the nitrous acid.

SECTION VI.

Objections to the preceding Experiments considered.

HAVING never failed, when the experiments were conducted with due attention, to procure some *acid* whenever I decomposed dephlogisticated and inflammable air in close vessels, I concluded that an acid was the necessary result of the union of those two kinds of air, and not water only; which is an hypothesis that has been maintained by Mr. Lavoisier and others, and which has been made the basis of an intirely new system of chemistry, to which a new system of terms and characters has been adapted. The *facts* that I alleged were not disputed; but to my *conclusion* it was objected, that the acid I procured might come from the phlogisticated air, which in one of my processes could not be excluded; and that it was reasonable to conclude that this was the case, because Mr. Cavendish had procured the same acid, viz. the nitrous, by decomposing dephlogisticated and phlogisticated air with the electric spark. In other cases it has been said, that the *fixed air* I procured

cured came from the *plumbago* in the iron from which my inflammable air had been extracted.

With respect to the former of these objections I would observe, that my process is very different from that of Mr. Cavendish; his decomposition being a very slow one by electricity, and mine a very rapid one by *simple ignition*, a process by which phlogisticated air, as I found by actual trial, was not at all affected; the dephlogisticated and inflammable airs uniting, and leaving the phlogisticated air (as they probably would any other kind of air with which they might have been mixed) just as it was.

I would also observe, that there is no contradiction whatever between Mr. Cavendish's experiment and mine, since phlogisticated air may contain phlogiston, and by means of electricity this principle may be evolved, and unite with the dephlogisticated air (or with the acid principle contained in it) as in the process of simple ignition the same principle is evolved from inflammable air, in order to form the same union; in consequence of which, the water, which was a necessary ingredient in the composition of both the kinds of air, is precipitated. That in other circumstances than those in which I made the experiments, the acid wholly escaped, and nothing but water was found, may be easily accounted for, from the small

quantity of the acid principle in proportion to the water, and the extreme volatility of it, owing, I presume, to its high phlogistication when formed in this manner.

In order to ascertain the effect of the presence of phlogisticated air in this process, I now not only repeated the experiment of mixing a given quantity of phlogisticated air with the two other kinds of air, and found, as before, that it was not affected by the operation; but I made the experiment with atmospheric air, instead of dephlogisticated. Since the air of the atmosphere contains a greater proportion of phlogisticated air, it might be expected that, if the acid I got before came from the small quantity of phlogisticated air which I could not possibly exclude, I should certainly get more acid, when, instead of endeavouring to exclude it, I purposely introduced a greater quantity. But the consequence was the production of much less acid than before, the liquor I procured being sometimes not to be distinguished from pure water, except by the greatest attention possible: for though the decomposition was made in the same copper vessel which I used in the former experiments, there was now no sensible tinge of green colour in it.

When I repeated this experiment in a glass vessel, I perceived, as I imagined, the reason of the small produce of acid in these new circumstances:
for

for the vessel was filled with a vapour which was not soon condensed, and being diffused through the phlogisticated air (which is not affected by the process) is drawn away along with it, when the exhausting of the tube is repeated; whereas, when there is little or no air in the vessel besides the two kinds which unite with each other, and are decomposed, the acid vapour, having nothing to attach itself to and support it (by being entangled with it) much sooner attacks the copper, making the deep green liquor which I have described. Sometimes, however, I have procured a liquor which was sensibly green by the decomposition of atmospheric and inflammable air, but by no means of so deep a colour, or so sensibly acid, as when the dephlogisticated air is used.

The extreme volatility of the acid thus formed (and which accounts for the escape of some part of it in all these processes) is apparent from this circumstance, that if the explosions be made in quick succession (the tube being exhausted immediately after each of them, and filled again as soon as possible) no liquor at all will be collected, the whole of the acid vapour, together with the water with which it was combined, being drawn off uncondensed in every process. I once made twenty successive explosions of this kind, in a copper tube, out of which I found that I drew thirty seven ounce measures

measures of air by the action of the pump, and found not a single drop of liquid, though near an hour was employed in the whole process, and the vessel was never made more than a little warmer than my hand. This was a degree of heat by no means sufficient to keep the whole of any quantity of water in a state of vapour, and is a circumstance that of itself sufficiently proves, that the vapour did not consist of water only.

Indeed, I think it impossible for any one to see this vapour in a tall glass vessel, and especially to observe how it falls from one end of it to the other, and the time that is required to its wholly disappearing, without being satisfied that it consists of something else than mere water, the vapour of which would be more equally diffused. If the appearance to the eye should fail to convince any person of this, the sense of *smell* would do it: for even in a glass vessel it is very offensive, though it might not be pronounced to be *acid*. I conjecture, however, that this, and every other species of *smell*, is produced by some modification of the acid or alkaline principle. Some may be disposed to ascribe this smell to the *iron* from which the inflammable air was produced; but the smell is the same, or nearly so, when the air is from tin, and would probably be the same if it were from any other substance.

Besides using atmospheric air, which contains a greater proportion of phlogisticated air, I have sometimes used dephlogisticated air which was not very pure; and in this case I have always observed, that the liquor I procured had less colour, and was less sensibly acid.

These observations might, I should think, satisfy any reasonable person, that the acid liquor which I procured by the explosion of dephlogisticated and inflammable air in close vessels did not come from the phlogisticated air which could not be excluded, whether it was that which remained in the vessel after exhausting it by the air pump, or that with which the dephlogisticated air was more or less contaminated.

I must observe, that the supposition of water entering into the constitution of all the kinds of air, and being, as it were, their proper *basis*, that without which no aëriform substance can subsist (which the preceding experiments render in a high degree probable) makes it unnecessary to suppose, as myself as well as others have done, that water consists of dephlogisticated air and inflammable air, or that it has ever been either composed or decomposed in any of our processes.

That water is decomposed when inflammable air is procured from iron by steam, is not probable; since the inflammable principle may very well be supposed

supposed to come from the iron, and the addition of weight acquired by the iron may be ascribed to the *water* which has displaced it. Also when the *scale of iron*, or *finery cinder*, is heated in inflammable air, it gives out what it had gained, viz. the water.

The most plausible objection to this hypothesis is, that iron gains the same addition of weight, and becomes the same thing, whether it be heated in contact with steam, or surrounded by dephlogisticated air. But from the preceding experiments it appears, that by far the greatest part of the weight of dephlogisticated air is water; and the small quantity of acid that is in it may well be supposed to be employed in forming the *fixed air*, which is always found in this process: for that there is one common principle of acidity, and that all the acids are convertible into one another (at least the nitrous acid into fixed air) is by no means an improbable supposition, though we are not yet in possession of any process by which it may be done. It is pretty evident that, in this respect, nature actually does what we are not able to do.

In reply to what has been objected to my former experiments, as being liable to exception from the phlogisticated air which could not be excluded from the dephlogisticated air when it was decomposed by means of inflammable air, I would farther observe,

serve, that I have found the process I have made use of to have no tendency whatever to decompose phlogisticated air. Indeed, nothing that we have hitherto known concerning this kind of air could make it probable, that mere *heat*, in contact with dephlogisticated or inflammable air, *could* have this effect. And it is of no consequence whatever to say, that any particular substance, imagined to be decomposed, is *present* in a process, unless it can be shewn that, in that process, there are agents capable of decomposing it. If mere *heat* (which is all that my process requires) would decompose phlogisticated air, and reduce it to nitrous acid, the transmission of common air (which consists of dephlogisticated and phlogisticated air) through a red hot tube would have this effect, which it is well known not to have.

But what I have asserted above is a conclusion which I have drawn from comparing the decomposition of dephlogisticated air by the two processes with nitrous and inflammable air. That nitrous air, when mixed with dephlogisticated air, has no tendency to produce phlogisticated air, is evident from the almost total evanescence of both of them, when they are very pure, and mixed in due proportions; and that nitrous air has no effect on phlogisticated air is well known. If then the
firing

firing of dephlogistified and inflammable air had a tendency to decompose any portion of phlogistified air, which should happen to be mixed with them, less would remain after the firing of inflammable and impure dephlogistified air than after mixing it with nitrous air; for as the impurities of dephlogistified air consist of phlogistified air, those would disappear in a greater proportion in the former process than in the latter. But by many careful trials I find, that I can reduce any kind of dephlogistified air no farther by a mixture of inflammable air than I can by nitrous air. When the proportions are well managed, the diminution is as nearly as possible the same in both the cases.

I must observe, however, that it requires more nitrous air than inflammable air (from iron by steam) to produce this effect in the proportion of about ten to nine; so that nitrous air does not contain quite so much phlogiston as an equal bulk of inflammable air, as I had before thought to be the case.

In this section it will be observed, that I make the diminution of common air by nitrous air to be considerably less than I have usually done before. This has been the consequence of giving the two kinds of air a little agitation at the instant of mixing,

ing, which will generally make the diminution less by two tenths of a measure. But I have found, that when these mixtures of air, with and without agitation, have been kept some time, they approach to an equality of bulk.

At the same time I have observed, what I think not a little extraordinary, that agitation prevents the greatest diminution of dephlogisticated and nitrous air. I have found it to be 2.5 without agitation, and 6. with it.

The less diminution of the mixture of nitrous and common air is probably owing to the presence of so much phlogisticated air, which impedes the meeting of the nitrous air with the dephlogisticated air in the mixture; because I find the same to be case when I mix the same proportion of inflammable air with dephlogisticated air; and when dephlogisticated air is agitated with nitrous air, the water may impede their union, as the phlogisticated air did before.

There is, therefore, no source of the *nitrous acid* which I find on the decomposition of dephlogisticated and inflammable air, besides the union of of those two kinds of air, which therefore do not make *mere water*, as the antiphlogistians suppose.

N. B. To this article, when originally printed in the Philosophical Transactions, were subjoined letters

ters to me from Dr. Withering and Mr. Keir, containing analyses of the green liquor formed in the experiments here mentioned. They may be seen, Phil. Transf. Vol. 78, p. 323.

Before I conclude this section, I shall just mention a few circumstances attending the many explosions I have made of inflammable and dephlogisticated air in the long metallic and glass vessels I have made use of, as they were pretty remarkable. The explosions were made by a small electric spark at one end of the vessel, and the greatest force of the explosion was always at the other end. No tinned iron vessel could bear many of them before they swelled out at that end, and at length burst; and even the flat end of the copper vessel, which was not less than one tenth of an inch thick, was in time made quite convex, and the cylindrical part next to it was made very sensibly wider than any other part of the tube. This must have been effected by mere *force*, and not by *heat*; for the hottest part of the tube after every explosion, was never there, but always about the middle, though something nearer to that end than the other, and in the glass vessel the dense cloud was always formed at that end.

The probability is, that the air where the electric spark is made taking fire first, the inflammation does not extend itself so rapidly but that the air at the
opposite

opposite end is first condensed, in consequence of the inflammation and expansion of the air at the other end, so that the air is there fired in a condensed state ; and hence its greater force.

SECTION VII.

*Of Air produced by the Solution of Vegetable Substances
in Spirit of Nitre.*

THE experiments, of which an account will be given in this section, were occasioned, in part, by a hint thrown out by Mr. Bewley, in his letter to me, printed in an *Appendix* of one of my former volumes ; but more immediately by an experiment which I had the pleasure to see at Paris, in the laboratory of Mr. Lavoisier, my excellent fellow-labourer in these inquiries, and to whom, in a variety of respects, the philosophical part of the world has very great obligations.

Mr. Bewley says, that he had always taken it for granted, that the elastic fluid, generated in the pre-

paration of nitrous ether, without distillation, was fixed air ; but that, after seeing the first publication of my papers relating to air, he found, on examination, that it had the general properties of nitrous air.

At Mr. Lavoisier's I saw, with great astonishment, the rapid production of, I believe, near two gallons of air, from a mixture of spirit of nitre and spirit of wine, heated with a pan of charcoal ; and when that ingenious philosopher drew this air out of the receiver with a pump, and applied the flame of a candle to the orifice of the tube through which it was conveyed into the open air, it burned with a blue flame ; and working the pump pretty vigorously, he made the streams of a blue flame extend to a considerable distance. Being very much struck with this experiment, I determined with myself to give particular attention to it, and pursue it after my return to England.

My first idea was, that this air was the same thing with the phlogisticated nitrous air, as I then called it, which I had procured, by exposing pieces of iron or liver of sulphur to nitrous air, the phlogiston of the spirit of wine being, as I supposed, disengaged in this process, and becoming incorporated with the nitrous acid, in the same manner as the phlogiston that is disengaged from the other two substances. These kinds of air differed, however, in one respect, viz. that

that in Mr. Lavoisier's experiment the flame was blue, whereas it had not been so in mine. But this seemed to be a circumstance of no great importance. Indeed I cannot say, that, at present, my idea of the thing is materially different from what it was then; but I have since had an opportunity, by pursuing this experiment, of observing a much greater variety in the production of air by means of spirit of nitre, than I had any expectation of before.

In general, it will be seen, in the course of these experiments, that if the substance with which the spirit of nitre is heated, whether it be fluid or solid, contain much phlogiston, the air produced from it will be nitrous air, or possess the property of diminishing common air to a considerable degree; and, in almost all cases, with a mixture of fixed air. If the substance be inflammable, the air will generally be such as I saw at Mr. Lavoisier's, burning with a blue flame. But this inflammability is of a very delicate kind, resembling that of dephlogisticated nitrous air; for the air is easily deprived of it by washing in water.

A particular account of these experiments, though very remarkable in their nature, will, I foresee, be thought tedious by some persons; but the detail will be very useful to such as shall chuse to prosecute them; especially on account of the precautions that

I shall occasionally give to prevent disagreeable accidents from them. Every chemist knows how hazardous it is to mix spirit of nitre with inflammable matters; and I was not unapprized of it, having seen the effect in a course of chemical lectures many years ago. But, being obliged to make these mixtures in a very different manner, the effect could not be obviated without a variety of precautions, which experience only taught me.

Beginning with *spirit of wine*, in imitation of the experiment which I had seen at Mr. Lavoisier's, I made the mixture with the spirit of nitre, in the manner directed in the process for making nitrous ether; putting about one third of spirit of nitre to two thirds of spirit of wine, in such a phial as e, Pl. I. mixing them very gradually. Heating this mixture with the flame of a candle, I received the air in water; and when I had procured a considerable quantity of it, I examined it, and found it to burn with a gentle blue, or greenish flame, nearly the same, as well as I could recollect, with that which I had seen at Mr. Lavoisier's; so that I had no doubt but that my process, though somewhat different from his, had answered perfectly well.

Considering this flame with attention, I thought it very much resembled that which is produced by a mixture of about one third inflammable air, and two thirds nitrous air; and concluded, that it was probably

probably composed of them both; the nitrous acid forming nitrous air, by seizing upon the phlogiston of the spirit of wine; and there being a redundancy of inflammable matter, sufficient to render the air partially inflammable.

In the directions to make nitrous ether, I was cautioned to pour the spirit of nitre upon the spirit of wine, and by no means to pour the spirit of wine upon the spirit of nitre. But though this method of mixing these liquids may not answer the purpose of making nitrous ether, it answered very well for the production of air, and was a very useful variety in the process. It is necessary, however, that the unexperienced operator should be upon his guard in these experiments.

The spirit of nitre should be much diluted, and the quantity of any liquid inflammable matter should be very small, just sufficient to cover the surface of it: otherwise, though the mixture may exhibit no alarming appearance at first, it will, in a little time, become very black, beginning at the surface; the phial will then be filled with red fumes, the air will be generated in a prodigious torrent, and, unless the tube through which it is transmitted be sufficiently wide, and the vessel in which the mixture is made be very strong, the whole will be exploded with great violence. Of this I have seen but too many instances; and sometimes when I had thought that

my experience had taught me sufficient precaution. Besides, all oily matters become extremely viscid, by mixing with spirit of nitre ; and this viscid matter getting into the tube, stops it up, and much increases the hazard of an explosion. But to recur to the experiments.

Having poured a very little *spirit of wine* upon a quantity of diluted spirit of nitre in a glass phial, with a ground stopper and tube, a great quantity of air was presently produced. When a candle was dipped into this air, it was extinguished ; but in going out was surrounded with a slight blue or green flame, but hardly more than is perceived in nitrous air. Almost one half of this produce of air was readily absorbed by water, and precipitated lime in lime water ; and I doubt not but that, in the subsequent experiments, as well as in this, a great proportion of the air produced in this manner was fixed air. The remainder was nitrous, almost as strong as any.

Upon air produced in this manner from *oil of turpentine*, I happened to make a few more experiments, some of which are not a little remarkable. When I used the strongest spirit of nitre in this process, it was very difficult to get much air, on account of the suddenness of the effervescence ; but a great quantity of air is easily produced by diluting the smoking spirit of nitre with an equal quantity of water.

water. At one time, however, when I had heated this mixture pretty much, and it had yielded a great deal of air, though I withdrew the candle, the air continued to be produced faster and faster for about a minute. It then came quite in a torrent ; all the oil of turpentine was thrown out of the phial, and the spirit of nitre only left in it. This is likewise the case with other similar mixtures ; so that when it is necessary to apply heat, it should be done very gradually and cautiously, and the air should never be generated very fast, unless the purpose of the experiment require it, and the operator be upon his guard accordingly.

When I received this air in water, it extinguished a candle, and did not diminish common air. When received in quicksilver, it still extinguished a candle ; but as it went out the third or fourth time, it was surrounded with a bluish flame, as large as that of the candle. And happening, at one time, to apply more heat than I intended when the air was received in water (and in consequence of it, the air was produced very suddenly) I examined it immediately, and a candle burned in it with an enlarged flame, though not remarkably so. It shews, however, that in this process also, as well as in the process for making dephlogisticated nitrous air, the property of its admitting a candle to burn with an enlarged flame depends, in a great measure, upon

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the *time* at which the experiment is tried after the air is produced, and upon other delicate circumstances.

A quantity of this air, received in water, was about half absorbed in one night. By agitation it appeared to be absorbed not so readily as fixed air, nor with so much difficulty as nitrous air, but in a medium between both. When this air was reduced to about one eighth of its original bulk, it was diminished by nitrous air. But this is the case with all the kinds of air that will bear the experiment, and even with nitrous air itself, as I have observed before.

At the time that I made the preceding experiments with oil of turpentine, I had no lime water at hand; and therefore only judged that part of the produce was fixed air, by the manner in which it was absorbed by water. But, less certain as this test is, a person much used to experiments of this kind, will be able to apply it with sufficient certainty in most cases. However, repeating this experiment, when I had procured the glass phials with ground stoppers and tubes, I found that the greatest part of this air was unquestionably fixed air, precipitating lime in lime water, as much as any fixed air whatever, and that the remainder was strongly nitrous. Attempting at this time also, to receive the air in quicksilver, a good deal of the vapour of the spirit of
↓ nitre

nitre came over; and, dissolving the quicksilver, made the produce of air almost wholly nitrous.

I observed, at one time, when I had produced this air in a phial with a ground stopper, that after the first part of the process, in which no heat was applied, the water rushed back into the phial. Upon this I applied the flame of a candle to the diluted mixture, and getting a second produce of air, examined them both separately. Both of them contained a great proportion of fixed air, precipitating lime in lime water very much; and when the fixed air was washed out of them, they both diminished common air, but the latter more than the former. Two measures of common air, and one of this, occupied the space of little more than two measures.

In order to judge how far an *acid* prevailed in this air from spirit of nitre, and oil of turpentine, I put alkaline air to it; when instantly a white cloud was produced, which rose to the top of the vessel; but it was by no means so dense as that which is produced by mixing alkaline air with any of the acid airs; nor did the whole quantity of air disappear, but only half of it. However, all the inside of the tube was covered with a saline substance, which I did not examine, but supposed it to have been the *nitrous ammoniac*. Having the curiosity to dip the
flame

flame of a candle, which happened to be at hand, into the air that remained of this mixture, it appeared to be so far inflammable, as even to make a considerable explosion ; but not quite so great a one as I have observed to have been made by a quantity of dephlogisticated nitrous air.

Repeating this experiment some time afterwards, about one fourth of the mixture of this air, and alkaline air, disappeared upon their being put together. Half of the remainder was absorbed by water ; and in this second remainder (which, by its redness, on being exposed to common air, appeared to be considerably nitrous) a candle burned with a beautifully enlarged flame.

In these cases the alkaline air must have supplied the phlogiston. For neither of the component parts of this air, viz. the fixed or the nitrous, are either separately, or together, inflammable. It is something remarkable, however, that when I mixed equal quantities of nitrous and alkaline air, and examined the mixture immediately, the nitrous air seemed not to have been at all affected by the alkaline air. It was not in the smallest degree inflammable. I had imagined that alkaline air might, in this manner also, have phlogisticated the nitrous air ; but it seems that when it is so applied, it has no such effect.

Air

Air produced from all the *essential oils* by spirit of nitre, has, I believe, the same properties as that which is produced from oil of turpentine. I tried another, but I forget which, in a phial with a ground stopper, and the air produced from it precipitated lime in lime water, extinguished a candle, and diminished common air a little.

Ether, both vitriolic and nitrous, heated in spirit of nitre, yields the same kind of air as the essential oils, or spirit of wine, viz. partly fixed air, and partly dephlogisticated nitrous air. Equal caution is also necessary in conducting this process; for the phenomena attending it are the same that I described in the beginning of this section, and in the highest degree. I would therefore recommend the using of a very small quantity of the ether, and putting it upon the spirit of nitre.

At first, however, in imitation of the process for making nitrous ether, I poured the spirit of nitre upon the ether, as I had done at first also with spirit of wine; and, heating the mixture, received the air, which it yielded in great plenty, in quicksilver. This air made no cloud with the mixture of alkaline air; it burned exactly like the vapour of ether itself; and when part of the mixture had boiled over, it quickly absorbed the air that had been generated.

Seeing

Seeing sufficient reason to disapprove of this process, I had recourse to the other, and found that when I used a very diluted spirit of nitre, and but little ether, the experiment was much more manageable, and the air was produced in sufficient plenty. This air was readily absorbed by water; and upon putting alkaline air to it, a very slight cloud rose to the top of the vessel; but there was no sensible diminution of the quantity of air occasioned by it. When a candle was dipped into this air, it was extinguished many times, but always with a beautiful bluish flame, much larger than the natural flame of the candle. Towards the close of the experiment, the air in the inside of the vessel became red; a certain sign of its being considerably nitrous. On repeating this experiment, when I had procured the phials with the ground stoppers and tubes, I had the most satisfactory proof, that part of this produce of air was fixed air, by its precipitating lime in lime water; and that the remainder was nitrous, almost as strong as any, by its power of diminishing common air.

The result of the experiment with *nitrous ether* was, in all respects, the very same as that of this with vitriolic ether. I made the experiment, because it might have been expected that there would have been some difference in the result, as the ni-
trous

trous ether is the produce of spirit of nitre, with which it was now mixed.

Spirit of nitre, heated with *olive oil*, yields the same kind of air with that which is produced from essential oils, &c. but the process is exceedingly troublesome, owing to the tenacity of the oil; and it is not much more manageable, when but a very little of the oil is put to a large quantity of the diluted spirit of nitre. The air which I got in this manner precipitated lime in lime water.

With very great difficulty I got, in a phial with a ground stopper, a very small quantity of air from spirit of nitre and *tallow*, the water rushing into the vessel after every gush of air. It precipitated lime in lime water.

The result of the experiment with *bees wax*, was the very same with that with tallow. Putting a small piece of bees wax upon a quantity of pretty strong spirit of nitre, I got air which made lime water turbid; but not enough to ascertain its other properties. This process was equally difficult with the preceding, on account of the water rushing into the phial after every gush of air.

I had the curiosity to endeavour to procure air from some of the *gums*, &c. by this process, and found the result to be, in the main, the same with that of the preceding experiments.

Gum-

Gum-arabic easily dissolves in the nitrous acid; and as it dissolves, a great quantity of air is produced, making a beautiful appearance; but when the acid is nearly saturated, it becomes viscid, and the vessel gets full of froth. Part of this air was fixed, precipitating lime in lime water, and being readily absorbed by water. The remainder was nitrous, almost as strong as any.

The result was the same with *gum copal*, excepting that this substance did not sink in the spirit of nitre, as the gum arabic had done.

Camphor, with diluted spirit of nitre, yielded very strong nitrous air; but required a considerable degree of heat. A good deal of the camphor, which had been fluid, and had swum on the surface of the spirit of nitre, came over, and resumed its natural appearance in water. I did not try whether any part of this produce was fixed air.

I got some air by spirit of nitre from *amber*, which precipitated lime in lime water; but the quantity was too small to be examined any farther. Afterwards I got a larger quantity from a greater number of small pieces of amber, heated in a weak spirit of nitre, contained in a phial with a ground stopper. About one third of this produce was fixed air, precipitating lime in lime water, and being readily absorbed by water. In the remainder a candle burned with an enlarged greenish flame. It
also

also diminished common air; so that two measures of common air, and one of this, occupied the space of two measures and a quarter.

N. B. Most of the pieces of amber used in this experiment were turned black quite through, the rest continuing of their natural colour.

It happened, in the course of these experiments, that a bit of *sealing wax* got into the phial, and I observed air to issue from it very copiously. Upon this, I put a piece of sealing wax into the phial, with spirit of nitre, and received the air at different times. That which came over first was, in the highest degree, nitrous; but when, with the application of more heat, I caused a copious production of a very turbid kind of air (which however, presently became transparent) it hardly affected common air at all. It was then pretty readily absorbed by water; and though at first it extinguished a candle, yet when it had been washed in water, a candle burned in it with a blue flame. Indeed when the candle was extinguished in it, it went out with that kind of blue flame. The course of this experiment will be found to be analogous to that with other *hard substances* containing phlogiston, which I shall now recite, though many of them were made before this.

Having found that *charcoal* would dissolve in oil of vitriol, and thereby yield a vitriolic acid air, I had the curiosity to try what would be the effect
of

of an attempt to dissolve this substance in spirit of nitre. This was when I had made but little progress in the preceding experiments with oily and gummy substances, and I had no expectation of the result. I began with taking the produce in quicksilver, as I had done with that from the vitriolic acid; but all that came over in this manner, was the nitrous acid vapour, which, seizing upon the quicksilver, produced nitrous air.

After this, I received the produce in water, and found it to be genuine nitrous air, almost as strong as any that is produced from metals. At that time I was much surprized at this result, having imagined that nitrous air could not be procured but by the solution of metals in spirit of nitre; and I considered this as another property in which metals and charcoal resemble each other; besides those which I had noted before, and an account of which may be seen in a paper formerly printed in the Philosophical Transactions. But presently after this I got nitrous air equally strong from other hard substances, such as *dry wood* of various kinds, &c. but in these processes the quality of the air differs exceedingly, according to the *degree of heat* applied, and other circumstances: and I think the subject deserves a farther investigation. To promote this, I shall recite the principal facts of this kind that have occurred to my observation.

Having

Having poured about a quarter of an ounce measure of smoking spirit of nitre, mixed with an equal quantity of water, upon some *pounded charcoal*, and having applied to it the flame of a candle, I collected a large jar full of air, in all twenty eight ounce measures. When about half of this quantity of air was produced, it was impossible to apply any more heat, but the spirit of nitre would come over; which it did, tinged with a deep black. When all the liquor was come over, still one fourth part of the air was produced with the application of a strong heat. The air of this whole produce, which was not taken at different times, was strongly nitrous. Two measures of common air, and one of this, occupied the space of no more than two measures.

It was my seeing this air produced in different circumstances, viz. before any of the acid came over, and afterwards, that suggested to me the importance of taking the air at different times, according to the change of circumstances in the production of it; a hint which I pursued to very great advantage afterwards, as the reader has already seen, and will see farther, in the course of my experiments.

Repeating the experiment with this view, I examined the first produce of air, which came over while the heat was very moderate, and found it to

be very strong nitrous air, almost as strong as that which is procured from metals. Towards the last I increased the heat, and by that means produced a very turbid air, of which I collected a prodigious quantity. Sometimes, however, the air would be quite transparent, and then turbid again, several times. I endeavoured to take the turbid air and the transparent separately, and I succeeded pretty well; but I found them both to be of the same quality, extinguishing a candle, and diminishing common air but very little; two measures of common air, and one of this, occupying the space of little less than three measures.

At this time I made use of the phials represented fig. *a*, Pl. I. with common corks; and observing that the corks were always much corroded in these experiments, I thought it would be proper to ascertain the effect of the spirit of nitre on the *cork*, in order to make proper allowance for this circumstance in future experiments. I therefore poured a quantity of spirit of nitre on some pieces of cork, and treating it in the manner above-mentioned, I found the produce of air to correspond very exactly with that which I had got from the charcoal. With a moderate degree of heat the air was strongly nitrous; and with a great heat the air was turbid, and much less nitrous. I was not a little surprized to find that nitrous air was produced from cork, as
it

it intirely overturned my system of the production of this air, depending upon that property of the charcoal by which it resembles metals. However, I presently found, that genuine nitrous air was produced from a variety of other *hard substances*; for at that time I had not discovered that it was produced from any liquid ones. The correspondence of an experiment which I made with old dry oak with that which I made with charcoal is striking enough; and one of them may a little illustrate the other.

I put about half an ounce measure of the raspings of *old dry oak* into one of the phials above-mentioned, fig. *a*, and poured upon them as much spirit of nitre, half diluted with water, as made them thoroughly moist. Air was instantly produced, without the application of any heat. This air I received, together with a little that was produced by holding the flame of a candle, at the distance of about a quarter of an inch, from the side of the phial. I then placed the candle nearer, and received the air at five different times; the last but one being produced when the flame touched the side of the phial, and the last, when it was placed close under it, and after all the moisture seemed to be expelled from the phial. The first produce was of the nature of nitrous air, the two next much more so, almost as strong as any; but

the two last were hardly nitrous at all. A candle went out in this air, burning with a bluish flame, as if it had been in part a mixture of inflammable, nitrous, and fixed air. That part of this produce was fixed air, was evident, by its being readily absorbed by water; but I did not apply to it the test of lime water.

Seeing this astonishing difference in the produce of air by spirit of nitre from different substances, and even from the same substance in different circumstances, I thought that it might be possible, by this means, to distinguish those substances that are *nutritious* from those that are not; and, in my imagination, I had thought it possible to ascertain the *quantity of nutriment* that different substances would yield by the quality and quantity of the air produced from them; but the experiments by no means answered such fond expectations. I found, however, what I did not expect, viz. a most remarkable difference between the air produced from *animal substances* of several kinds, and from *vegetables*: for, in general, the former had little of the nitrous property; but the latter, though nutritious, yielded the same kind of air with that which I had got from wood or charcoal. The facts surprized me very much, and I can give the reader no clue to lead him through the labyrinth.

The

The vegetable substances which I tried were *wheat flour*, *barley*, and *malt*, all of which yielded nitrous air in the first part of the produce, and air of the same quality with the last produce from charcoal, if the process was continued a long time, and with a strong heat. I had once suspected that the nitrous quality might have come from the cork with which the phial was closed; but I was satisfied that it came from the substance within the phial, when, instead of a phial closed with a cork as before, I used one of those represented fig. *b*, which I have observed to have been contrived by Mr. Vaughan. Having put the barley and spirit of nitre into this vessel, I heated it in a vessel full of water, placed on the fire, covering the phial with a glass jar filled with water, in order to receive the air. The air procured in this manner was still strongly nitrous, though it could come from nothing but the spirit of nitre and barley.

As I attended to a few collateral circumstances in the experiment with the *malt*, it may be worth while to recite the particulars. Having just covered one pennyweight of malt with diluted spirit of nitre, I made it boil, and procured from it two jars full of air, each containing near thirty ounce measures, and I might have collected more. That which came first, and which was transparent, diminished common air almost as much as the strong-

est nitrous air. The air which came last, and which was turbid, hardly diminished common air at all, and it was readily absorbed by water. Before it was agitated in water, it extinguished a candle; but afterwards, when it was reduced to about one fourth of its original quantity, a candle burned in it with a lambent blue flame.

N. B. Towards the close of this process, part of the contents of the phial were reduced to a coal.

SECTION VIII.

Of Air procured by the Solution of Animal Substances in Spirit of Nitre.

I Profess not to be able to assign any reason for the difference in the produce of air from *animal* and *vegetable* substances; but the experiments, of which an account will be given in this section, compared with those recited in the last, will prove, that, in general, there is a very considerable one.

It has been seen that vegetable substances, dissolved in spirit of nitre, besides fixed air, yield
nitrous

nitrous air, and frequently as strong as that which is procured by the solution of metals in the same acid; and this is the case whether the spirit of nitre be much concentrated, or much diluted. On the contrary, animal substances, in general, treated in the same manner, yield about the same proportion of fixed air; but the residuum is either not at all, or in a very slight degree, nitrous (except in some cases where the spirit of nitre is very strong) but is a kind of air which, neither affecting common air, nor being affected by nitrous air, but simply extinguishing a candle, may be termed *phlogisticated air*. Towards the end of a process, indeed, when, by means of a strong heat, the produce of air is very rapid, and the air full of clouds, it is, like air produced from vegetable substances in the same circumstances, slightly inflammable, burning with a lambent, greenish, or bluish flame.

As there is a considerable variety in the result of these processes, arising from several circumstances, the influence of which may not be apprehended, I have been careful to note every thing relating to them, that appeared to me at the time to be of any importance. But, notwithstanding this, it is very possible I may have made omissions, of the effect of which I was not apprized; and therefore those who shall endeavour to repeat the experiments after me may not find precisely the same results

that I have reported. This will often be the case in experimental inquiries so new as these; and as no human care has yet been sufficient to prevent this inconvenience, it is the part of human candour to make proper allowance for it.

I cannot help flattering myself, however, that these experiments, properly pursued, may be a means of throwing light upon the two great natural processes of *vegetation* and *animalization*; as they exhibit a new and striking difference between substances formed by them. On this account I would willingly recommend them to the particular attention of chemists and physicians. The experiments themselves, nearly in the order in which they were made, are as follows.

I put equal quantities of spirit of nitre and water upon some pieces of *beef*, dried till they were perfectly hard, but without being burned, and took the first produce of the air, which was generated without the application of heat, and was very considerable; and afterwards that which came over when the flame of a candle was placed within about a quarter of an inch from the phial; but neither of them sensibly affected common air. They were both pretty readily absorbed by water, and extinguished a candle. I had expected that this air, like that from dry wood, would have been nitrous air.

This

This experiment being made with the fleshy part of a *muscle*, I next took a *tendon* from a neck of veal, imagining, from its firmer texture, that the air produced from it might approach nearer to that from wood; but the air that came from it neither diminished common air, nor was diminished by nitrous air, nor was it readily absorbed by water, and a candle went out in it. It seemed, upon the whole, to be much the same thing with phlogisticated air.

I thought there might be some difference in this respect, between air produced from the *white*, and from the *brown* flesh of animals; but I made the experiment with the breast and leg of a *turkey*, without finding any. That which was produced from these substances exactly resembled the air that I had got from the tendon of a calf; except that it was more readily imbibed by water. I agitated a quantity of it in water five minutes, when one fourth of it was absorbed, but the remainder still extinguished a candle, and did not differ from what it was before, except that it was now diminished by nitrous air, like all other kinds of air agitated in water. When all the flesh was dissolved, air was still produced in great plenty, upon the application of the flame of a candle. The air produced in this manner was very turbid at first; but the quality of it was not sensibly different from

from that which came first, and which was transparent.

I repeated this experiment with the same event, observing that the turbidness of the air depended upon the *degree of heat* with which it was produced; for, after producing a large quantity of turbid air, I lessened the heat, and presently the air was transparent as at first, and on increasing the heat, the air was turbid again.

Having found no air of the nitrous kind from the flesh of an animal of the *quadruped* species, or of a fowl, I was willing to try what would be the produce from the flesh of *fishes*, *insects*, and *exanguious* animals.

From the flesh of *salmon*, made thoroughly dry, and then dissolved in spirit of nitre, I got a great quantity of air, at first without heat, till the whole was nearly dissolved; when about a quarter of an ounce measure of this solution still yielded more than a quart of air. At the last this liquor, which had been pretty clear, became suddenly opake; and in this state it yielded air the most plentifully, and continued to do so till, all the moisture being evaporated, it became a dry coal. While it continued clear, a strong heat, occasioned by applying the flame of a candle close to the phial, would immediately make the air turbid, especially toward the end of the process, just before the liquor became opake,

opaque. At this time, however, the air in the inside of the phial had nothing of that appearance, nothing being seen in it but the red fumes of the spirit of nitre ; but when the liquor became opaque, it was filled with very dense white fumes.

The air, in all the stages of this experiment, was in part fixed, precipitating lime in lime water. In the middle of the process the residuum was nitrous ; but only in a slight degree. Towards the conclusion it had no sensible effect on common air ; and at last it burned with a blue lambent flame, which continued a considerable time after I had withdrawn the candle by which it had been set on fire. In the air that came just before the last, a candle barely went out, surrounded by a slight flame of that colour.

Repeating the experiment, I found nothing nitrous, either in the first produce of air, before the flesh was dissolved, or afterwards ; and at this time I was particularly careful not to use any of the flesh that was turned black, or very brown, in drying ; having some suspicion that the nitrous property of the air in the preceding experiment came from such parts of the flesh, being then a kind of charcoal.

The flesh of salmon having a peculiar colour and flavour, I thought it would not be amiss to repeat the experiment with some other kind of fish, the

flesh of which was white and tasteless. I therefore took the *flesh of perch*, and dissolving it in spirit of nitre, I procured a large quantity of air, no part of which was nitrous; but a considerable part of it was fixed, precipitating lime in lime water. The greatest part of this air was produced after all the flesh was dissolved; and at the last, when I increased the heat, the air was turbid; but it did not sensibly differ from that which was produced at first, except that a candle went out in it with the flame slightly tinged with green.

A large *worm*, treated in the same manner, yielded air that was in part fixed, making lime water turbid. The residuum extinguished a candle, and was, in a small degree, nitrous; owing, perhaps, to something on its stomach; for I had only pressed out the contents with my finger.

Air produced from a number of *wasps*, dissolved in spirit of nitre, was partly fixed, and the residuum so far nitrous, that two measures of common air, and one of this, occupied the space of two measures and a half. When the flame of a candle was dipped into it, it burned with a greenish lambent flame.

I had next the curiosity to try what kind of air might be procured from the insensible *excrescences of animal bodies*, as *horn, hair, feathers*, &c. which are protruded from the body, and seem, at first sight, to be in a kind of intermediate state between vegetable

table and animal substances ; but they appeared to be more of an animal than of a vegetable nature, i. e. judging by the air which I had hitherto found those substances to give.

With spirit of nitre and *hair* I got a quantity of air, part of which was fixed, precipitating lime in lime water, and the remainder, not absorbed by water, which was about two thirds of the whole, was in a small degree nitrous.

From a *crow-quill* I got air of the same quality with that from the hair in the preceding experiment. This quill was black ; and thinking it possible (as the hair I had made use of was also in part black) that the nitrous property of the air might come from the phlogiston which produced that colour, I repeated the experiment with a *white feather* ; but the result was the same ; or, rather, the air in this case, was more nitrous than in the former. Two measures of common air, and one of this, occupied the space of two measures and a half. Had I used a much diluted spirit of nitre, it will appear probable, from the experiments recited at the close of this section, that the produce would have been less nitrous.

Air was easily procured by dissolving *horn* in spirit of nitre. Part of it was fixed air, precipitating lime in lime water ; but a very great proportion of it was not absorbed by water. In this residuum

there was nothing sensibly nitrous. That which came first extinguished a candle, without any particular appearance; but in that which came last, it burned with a beautiful blue lambent flame.

I had thought that, possibly, the *inside of an oyster-shell*, or *mother of pearl*, might, together with fixed air, yield a quantity of such phlogisticated air as had been produced in the preceding experiments; but when they were dissolved in spirit of nitre, they each of them gave very pure fixed air, without any greater residuum than is found in the solution of chalk in oil of vitriol.

Pieces of *ivory* dissolved in a very beautiful manner, in hot spirit of nitre, and yielded a great quantity of air, which, in every stage of the process, precipitated lime in lime water. The residuum was not nitrous, and extinguished a candle, without any particular colour of the flame.

To try the difference between the same substance, in a natural state, and after it was reduced to a coal by fire, I dissolved some *charcoal of ivory* in spirit of nitre, and found that it yielded plenty of air, the greatest part of which was fixed, and the residuum was considerably nitrous. When the air was produced very fast, the inside of the phial was filled with a white fume. This ivory had been kept in a red heat, covered with sand, about an hour.

Eggs.

Eggs do not rank with the substances above mentioned; but being the produce of an animal, and yet no proper part of one, I shall recite the experiments I made upon them in this place. Both the *white* and the *yolk* of an egg, which I tried separately, yielded a considerable quantity of air, when dissolved in spirit of nitre, and the difference between them was not sensible. In both cases part of the air was fixed, precipitating lime in lime water, and the residuum was so far nitrous, that two measures of common air and one of this, occupied the space of two measures and a half.

It occurred to me, that, possibly, other parts of the animal, and the different animal *secretions*, might yield a different kind of air from that which the muscles had yielded; and from the little that I have done in this way, I cannot help thinking, that the experiments deserve to be prosecuted farther.

From the *crassamentum of blood*, with spirit of nitre, I got great plenty of air, part of which was fixed, but no part nitrous. At last the air was turbid; and, as it is usual in this case, a greater proportion of it was fixed air. Towards the last also, when the blood was completely dissolved, the air was produced irregularly; for after an interval of about a quarter of a minute, there would be a sudden gush of about a quarter of an ounce measure of air;

air; but between these intervals the produce was equable.

Spirit of nitre put to the *serum of blood*, immediately turns it into a white coagulum. This yielded less air than most other substances, treated in the same manner. Part of it was fixed air, precipitating lime in lime water, and the residuum was not nitrous, and extinguished a candle without any particular appearance.

Milk was also immediately coagulated by a mixture of strong spirit of nitre, and yielded air, one third of which was fixed, precipitating lime in lime water; and the remainder was so far nitrous, that two measures of common air, and one of this, occupied the space of two measures and a quarter.

From *cheese*, which was pretty old, I got air, a great part of which was fixed, and the remainder considerably nitrous.

Mutton-gravy, with strong spirit of nitre, gave but little air, perhaps twenty times as much as its bulk. It was in part fixed, and the residuum not sensibly nitrous.

It has been seen, in a preceding section, that all *oily matters*, of a vegetable nature, yield nitrous air in very great plenty, and that the produce is exceedingly rapid; so that many precautions are necessary in conducting the experiments. On this account I began

began to use the same in my attempts to get air from *Hog's-lard*, but found them to be altogether unnecessary: for this substance is but little affected by very strong and hot spirit of nitre, on the surface of which it continues fluid, and yields but little air, perhaps four times its bulk. Part of this was fixed air, precipitating lime in lime water, and the remainder was so far nitrous, that two measures of common air, and one of this, occupied the space of less than two measures: that is, it was almost as strongly nitrous as that which is produced from metals.

It is something remarkable, that, of all animal substances on which I have made the experiment, that part which seems to be the most remote from a vegetable nature, and is peculiar to animals, should approach the nearest to the nature of a vegetable in the air which it yields when dissolved in spirit of nitre. This is the *medullary substance of the brain*.

From part of the *brain of a sheep*, dissolved in strong spirit of nitre, I got a quantity of air, about half of which was fixed air, precipitating lime in lime water, and the remainder was so far nitrous, that two measures of common air, and one of this, occupied the space of two measures and a quarter. When it was completely dissolved, and by a strong

heat, the air came over very turbid, and a candle burned in it with a lambent greenish flame.

I repeated the experiment with part of the same brain that was *boiled*, and with the same result; except that I did not continue the process so long. The residuum of this air, when the fixed air was washed out of it, was so much nitrous, that two measures of common air, and one of this, occupied the space of two measures and one fifth. This I tried with the last of the three portions of air that I took. The first and second were not so highly nitrous; and yet I am confident that all the three portions were wholly the produce of the solution, both the phial and the tube being filled with bubbles, in the form of froth, before I took any air at all.

After I had made these experiments, it occurred to me, that possibly, this difference in the produce of air from vegetable and animal substances might arise from some difference in the spirit of nitre. But though I found that, in consequence of the acid being more concentrated, or more diluted with water, a real difference was occasioned in the air, still very much depended upon the substances themselves, as will appear from the following experiments.

A piece

A piece of *boiled mutton*, dissolved in very strong spirit of nitre, yielded air, which was partly fixed; with the residuum so far nitrous, that two measures of common air, and one of this, occupied the space of two measures and one third. Dissolving a quantity of the same mutton, in the same spirit of nitre, diluted with an equal quantity of distilled water, I procured air, which was not half so much nitrous as that in the preceding experiment. With the same result I also made this experiment with the *white of an egg*, which gave air much less nitrous when dissolved in a diluted spirit of nitre, than in the former case.

In order farther to satisfy myself, whether the result would not be the same with *vegetable substances* also, I took some pieces of very *dry old oak*, and dissolved them in exceedingly weak spirit of nitre. I also caused the air, by means of heat, to be produced very rapidly; in which case the air is generally less nitrous, at least toward the close of an experiment, as the reader will have observed: but when the fixed air was washed out of it, the residuum was almost as strongly nitrous as any air that is produced by the solution of metals.

Ox gall dried dissolves in the nitrous acid with as much rapidity as vegetable astringent substances,

and yields great plenty of nitrous air ; whereas animal substances in general yield only phlogisticated air, with a mixture of lambently inflammable air, by the same treatment. Now it is remarkable that the gall is secreted from the *venal blood*, which, according to my theory of the use of the blood in respiration, is then loaded with phlogiston ; while other secretions are made from the *arterial blood*, which has discharged its surplus of phlogiston.

SECTION IX.

Of the dephlogisticated Nitrous Vapour.

IN my early publications, I said I had no doubt but that the nitrous acid might be exhibited in the form of air, and that experiments might be made upon it with a great prospect of making considerable discoveries, provided that any fluid substance could be found capable of confining it. I have since made several attempts to divest this acid of the water with which it is generally combined ;
but

but though I have been favoured by some unexpected circumstances, I have been far from succeeding to my wish.

That this acid is capable of existing in this dry form, I presently satisfied myself by an attempt to expel air from it, by the same process by which I had before expelled the marine acid air, from spirit of salt; viz. by heating the fluid in a phial, and receiving the air in quicksilver. For though the acid vapour very soon united with the quicksilver, yet the jar in which it was received being narrow, the saline crust, which was formed on the surface of the quicksilver, impeded the action of the acid upon it, till I had an opportunity of admitting water to the air that I had produced, and of satisfying myself, by its absorption, of its being a real *acid air*, having an affinity with water, similar to other acid airs.

In the first experiment that I made of this kind, the redness of the air did not appear immediately; but after some time, when it might be presumed that the nitrous vapour had produced nitrous air, by a solution of the quicksilver; and the redness, I suppose, to have been the effect of the mixture of this newly-generated nitrous air, with that portion of common air, which had been contained in the upper part of the phial, and which had been expelled by the acid vapour. I did not admit water to this

air till after an hour ; and even then it was sensibly diminished, some of the acid air not having been seized by the quicksilver. The last time that I made this experiment, in which I produced about two ounce measures of air, I admitted water to it as quickly as I could, and then one third of the whole was imbibed by it.

In my account of the process to procure dephlogisticated air from calcined flint, and also from talck, I have observed that between the produce of the phlogisticated and dephlogisticated air, there is a considerable interval, in which nothing comes over but the pure *vapour of the acid*, which is instantly and wholly imbibed by water. This circumstance gave me a fine and unexpected opportunity of making some experiments upon this vapour. For the orifice of the tube through which it was transmitted being plunged in water, and bending considerably upwards, I could easily put over it phials filled with any kind of air that could bear to be confined by water ; and the end of the tube rising a considerable way within the phial, the vapour must necessarily come into immediate contact with the air contained in it.

The first experiment that I made upon this vapour, in these circumstances, was with *nitrous air* ; and it appeared to have the same effect upon it that had been produced by liver of sulphur, viz. diminishing

nishing it till it was no more capable of affecting common air ; and the operation was exceedingly quick. Indeed the whole progress of this experiment is not a little remarkable. The moment that the phial of nitrous air was exposed to this vapour, it became white, then transparent, then red ; and, lastly, transparent again. I took one quantity of this air, when the whiteness had just gone off ; and found that it was but little different from pure nitrous air, diminishing common air almost as much. Taking another phial when it was quite red, one third of the quantity had disappeared, and its power of diminishing common air was about one half of what it had been. I then let another phial remain exposed to this vapour, till I perceived that the diminution would go no farther ; when only one twentieth of the original quantity remained, and this did not affect common air at all.

When this process is quick, that is, when the nitrous vapour comes very fast, the whiteness preceding the redness, on mixing the nitrous vapour with the nitrous air, can hardly be perceived, and the vessel containing the air becomes exceedingly hot, as well as the tube through which it is transmitted. I observed that the vessel containing nitrous air continued exceedingly red for about a minute, without any visible change of dimensions in the air ; after which it was suddenly diminished to about one

fourth of its original quantity, which resembles the process of the effervescence of iron filings and sulphur.

I exposed to this nitrous acid vapour, *common air*, *inflammable air*, and *fixed air*, and all of them for a considerable time, without making the least sensible alteration in any of them. It is possible that a longer continuance of the process might have affected them; but a great deal less time was abundantly sufficient for this acid vapour to produce its utmost effect upon nitrous air. It should seem, therefore, that though this acid vapour contained phlogiston enough to phlogistificate, presently and completely, a quantity of nitrous air, it does not contain enough to phlogistificate common air, at least, that it requires either more time to effect this purpose, or a different mode of application.

As phlogiston had produced no effect upon fixed air, except in one particular case, viz. from the effervescence of iron filings and sulphur, I did not absolutely expect that it would have been affected in these circumstances. Besides, I only exposed the fixed air to this vapour as it was expelled from the phial by the flame of a candle, when the vapour is not so copious as when it is expelled by a strong sand heat, surrounding the whole phial placed in a crucible.

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In the course of the experiments, I thought I saw reason to conclude that the nitrous acid air is naturally *colourless*, like the other acid airs. For I observed that, though the inside of the phial, and also of the tube, was very red, during the transmission of both the phlogisticated and dephlogisticated air, yet that in the intermediate state, when the pure acid came over, all the inside of the phial was transparent; or if there was any sensible colour, it was of a whitish cast. At the same time it was observable, that this acid vapour, mixing with any other kind of air, produced a red colour. As there was this redness in inflammable air, and other kinds of air, for some time after this vapour was admitted to them, and they afterwards became transparent, I expected that some alteration would have been made in them, but I was disappointed.

I would here observe, that the young operator ought to be very cautious in conducting this process, and especially to take care that the tube through which this acid vapour is transmitted be sufficiently wide; by which I mean that the hollow part of it should be about one tenth, or one twelfth of an inch in diameter. When, at one time, I was so incautious as to make use of a tube much smaller than this, almost capillary, some particles of the flint, as I suppose, got into it, and stopped it up. However, there was a violent explosion of the phial, and
of

of all its contents, by which I was exposed to some danger; but providentially, at this time, as upon many other occasions, I escaped without any hurt. But, in such a kind of business as this, nothing can be expected to be done without such risques.

SECTION X.

An Account of some Experiments made in Consequence of an Attempt to confine the Nitrous Acid Vapour by Means of Animal Oils.

MY readers will easily recollect, from my former publications on the subject of air, that my greatest *desideratum* was to exhibit the *nitrous acid* in the form of air, after having exhibited some other acids, and the alkaline principle, in that manner; which is so exceedingly convenient for a chemical examination and analysis. Since the idea first occurred to me, I have never once lost sight of it; being well aware of the unspeakable importance of it in such investigations as I

have been engaged in; and the farther I have carried my researches, the more important is the part that I find this acid to act in the system of nature.

A great part even of my first publications on this subject related to *nitrous air*, into which this acid principally enters; the observations contained in my second volume, exhibited this acid in a much greater variety of modifications, as being equally a principal ingredient, both in the most noxious and the most wholesome, the most pure and the most impure of all the kinds of air; and my experiments and observations relating to the same acid made no less a figure in my subsequent publications. And still the subject is so far from being exhausted, that all that has been hitherto investigated seems to be nothing more than an opening to something much greater than any thing that we yet know concerning it. But I hope that we have access to it now in so many different ways, that our nearer approaches will be much facilitated.

My first object, as a thing necessary to all the rest, was, as I have said, to contrive how to exhibit the nitrous acid in the form of air, free from any combination with water, and unmixed with any other kind of air; in order to which it was necessary to confine it by some fluid substance,
with

with which it had no affinity, as I had been able to do with respect to other kinds of air, by means of water or quicksilver; in order that I might introduce whatever substances I pleased into this air, through that fluid; that their affinities with this air might be examined, with the same ease and convenience that I have been able to do in other similar cases. But I believe there is no *fluid substance* in nature, with which the nitrous acid will not readily combine, so as to be absorbed by it. But though, on this account, I have not been able to succeed to my wish, I have had such other resources, that my endeavours have not been wholly without success; having been able to exhibit the nitrous acid *in the form of air*, and *without water*, and to keep it in this state as long as I please, but not without a mixture of common air, and loaded with phlogiston. My *desiderata* now are, to *separate the phlogiston from it*, and *intirely to exclude the common air*.

In an early period of my experiments, I was not without hopes of being soon in possession of such a fluid substance as would fully answer the views above-mentioned; expecting, as I then suggested, that some of the *animal oils* would sufficiently confine the nitrous vapour. But in this hope I found myself altogether disappointed; this vapour being, in fact, no more capable of being confined
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by these oils, than by any other fluid substance whatever. However, as the degree of success I met with was in consequence of my unsuccessful attempts to gain my point in this way, and the experiments that I made for this purpose are in themselves curious, and of some importance, I shall begin this part of my narrative with an account of them.

Hoping that *whale oil*, which is a cheap article, might serve my purpose of confining the nitrous acid vapour, as well as any other animal oil, I imagined that I had only to contrive how to raise this vapour. Merely heating the acid, I had found, would not answer; and therefore some more powerful means was necessary to separate it from its water, and expel it in the form of air. Now, reflecting upon the exceedingly rapid solutions of several of the metals in this acid strongly concentrated, or with a mixture of a very little water, and the phenomena attending the receiving of the air so produced in water (when I had observed very large bubbles to issue from the end of the tube that transmitted the air, but exceedingly small ones rising to the top of the jar) I caught the hint of getting nitrous vapour by this means. For the large bubbles, I was well satisfied, must have been the *nitrous acid itself* in the form of air, but presently absorbed by water; while the small bubbles were the *nitrous*

air which was the proper produce of the solution; the heat attending the solution having been the means of expelling a great quantity of superabundant acid, along with the air. By some rapid solution of this kind, therefore, I was sure to get a very great proportion of *nitrous vapour*, though mixed with a little nitrous air, for which I should have to make an allowance.

With these ideas, having considerable hope, but not without some fear of disappointment, I filled a number of small jars with my whale oil, and placed them, inverted, in a basin of the same; exactly as I had been used to do with quicksilver, for those kinds of air that are capable of being confined by it: and having a small quantity of strong nitrous acid in one of my phials, with a ground stopper and tube, I dropped my lump of bismuth into it, pouring after it a small quantity of water, to promote the solution, and eagerly expected the event. But alas! nothing followed, but exactly such appearances as I had before seen in water. For though the prodigiously large bubbles, which issued in torrents from the orifice of the tube communicating with the phial, were not, indeed, so quickly absorbed as they had been in water, they were presently contracted in their dimensions, and were reduced in bulk so much, as not to exceed that of the heads of small pins before they reached the top
of

of the jar. I then found that the proper nitrous vapour was absorbed by the *oil* as it had been before by the *water*; and that all the permanent air that I got in this way was the small quantity of nitrous air which the solution yielded.

Several circumstances, however, attended this experiment, which struck me very much at that time, and were, indeed, then altogether inexplicable to me; though later experiments have made the theory of them pretty easy. I shall mention them as they presented themselves, with the explanations that have occurred to me since.

The first visible effect of the admission of this mixture of nitrous vapour and nitrous air to the oil, was the *beating* of it exceedingly, and turning it *green*. But when it had stood to cool it became *red*, and coagulated; and when mixed with other oil, it separated from it, and sunk to the bottom, exactly like the ice of oil, and continued there, without mixing with the rest of the oil.

But what struck me most, and was a circumstance wholly unexpected by me, was, that when I had thrown out all the nitrous air that had been produced in the process, and had filled the phials again with the oil only, air kept issuing from every part of it very copiously; so that in a short time a considerable quantity, not less than one fourth of the contents of each jar, was separated from it, expelling

elling an equal bulk of oil. When the jars were left in a cool place, and consequently the whole mass of oil was coagulated, this process was necessarily at a stand; but when I brought the jars near the fire, so as to liquify the oil, the discharge of air was resumed, and went on as briskly as before.

As I could not, immediately upon seeing this phenomenon, form any idea of the nature of this air, and it required some time to collect a quantity of it, sufficient for a satisfactory examination of its properties; I kept viewing this production of air with a considerable degree of surprize and anxious expectation. Among other things, I thought it possible that this might be the very *nitrous acid air* that I had been so long in quest of, escaping from the oil, which had been over-saturated with it, like water strongly impregnated with fixed air; and that thus, in this most unexpected manner, I was in possession of all my wishes.

With this idea, the first thing that I tried, as soon as I had, by putting all the small quantities together (which I could do in a trough of oil that I had prepared for the purpose) gotten enough for the experiment, was to introduce to it a quantity of alkaline air. But there was no appearance of any thing like a white cloud procured by mixing them; so that it could not have been any pure acid vapour, and consequently that hope was entirely blasted.

ed. I also found that this air was not absorbed by water, as a nitrous vapour would certainly have been.

I then concluded that this must be *nitrous air*, with which, as well as with the nitrous vapour, this oil had been saturated; but on admitting to it a proper quantity of common air, there was neither any redness, or diminution of bulk, produced. I then imagined that it might possibly be *inflammable air*; the phlogiston of the oil, as well as of the nitrous air, and nitrous vapour, having contributed to it; but it extinguished a candle. And finding at the same time that it did not make lime water turbid, it appeared to be, in fact, mere phlogisticated air.

The manner in which this air was produced by this process, and which I did not understand at the time that I first observed it, is, in short, this. All oils readily decompose nitrous air, as well as imbibe nitrous vapour; and in all cases in which nitrous air is decomposed, it is reduced to the state of phlogisticated air. But still I am entirely at a loss to account for the production of such a quantity of this air from the oil thus impregnated, after the nitrous air produced by the solution of the metal had been thrown out; considering that it is only a very small proportion of nitrous

air that any fluid substance can be made properly to imbibe; whereas this phlogisticated air was sufficient, in quantity, to have been the residuum of ten times as much nitrous air as the quantity of all the oil used in the experiment. This production of phlogisticated air from oil will be seen exactly to resemble the production of a still greater proportion of nitrous air from water, treated in the very same manner; but then I am not able to account for one of the facts any better than the other.

N. B. When I made a quantity of alkaline air pass through this oil saturated with nitrous vapour, in order to mix it with the air which had issued from it, the oil, from being red, became almost quite black.

After this I impregnated all the kinds of oil with nitrous vapour, but in a manner different from this, and I shall give an account of the effects of this impregnation, which are sufficiently remarkable, in a future section, appropriated to that purpose.

Thus ended my first attempt to procure nitrous acid air; fruitless, indeed, with respect to my principal object, but not quite useless in itself, and preparing the way for such a degree of success as I have since met with.

S E C.

SECTION XI.

Observations on the Nitrous Acid Vapour itself.

BEING disappointed, as has been seen, in my expectations of confining the nitrous acid vapour by *animal oils*, it occurred to me, that, in lieu of this, it might not be wholly without its use, if I could shut up this vapour in dry glass phials, with ground stoppers. And though, in this method of procuring it, by the solution of bismuth, or other things with which it unites most rapidly, there is necessarily a mixture of *nitrous air*, it is inconsiderable in proportion to the quantity of pure nitrous vapour itself. And though a mixture of *common air* also would necessarily remain in the phial, it could only serve to dilute the acid vapour, and could not materially alter the properties of it. Also, if the mouths of the phials were small, they might be opened, and various substances admitted to the vapour, without much loss of the acid, especially as all acid vapours, I had reason to think, were heavier than common air.

Being obliged to content myself with these moderate views, I presently thought of an easy method of putting my design into execution. This was by making the solution in a tall phial, that there might be room for the ebullition of the acid, without any danger of its being thrown out; and having the tube, through which the vapour was transmitted, bent downwards, in order to be inserted into the mouths of the phials that were to receive the vapour. This contrivance perfectly answered my purpose. For I soon found that as the solution was so very rapid, and consequently the production of nitrous vapour in this process exceedingly copious, I could, by placing a number of dry phials in a line, fill them all with this red vapour in the space of a few seconds; closing some of them while I was filling others; and when this was done, I could either keep the phials filled with the vapour itself, as long as I pleased, or put into them such substances as I intended to imbibe the vapour. Or, if I thought proper to do so, I could first put those substances, whether solid or liquid, into the phials, and then throw a stream of vapour upon them.

Though this vapour, in the manner in which I procured it, was always red, I have little doubt but that the nitrous acid itself, divested of every thing foreign to it, is as transparent as the other acids. But in this process it necessarily acquires, and carries

ries off with it, a great portion of phlogiston, from the metals that it has dissolved. And other observations shew that it is only when the nitrous acid becomes, in some measure, phlogisticated, that it assumes a deep orange colour; and that the less phlogiston it has, the paler it always is.

When a phial is filled with this red vapour from spirit of nitre, and closed with a ground stopper, or when it is confined in glass tubes hermetically sealed at both ends, which I have frequently done, it will continue red I do not know how long; but I have kept it many months in this state, and imagine it will never change its colour, except in consequence of phlogisticating the common air contained in the phial; when the phlogiston, which occasions the redness of this vapour, quits it, and loses its colour, by being incorporated with common air, as will be seen presently.

The change of colour given to this vapour by heat is not a little remarkable, for it is altogether independent of gravity or condensation. In order to make some experiments of this kind to proper advantage, I procured a glass tube, three feet long, and about an inch wide, closed at one end, and fitted with a ground stopper at the other. This tube I easily filled with red vapour, in consequence of its being much heavier than common air; and closing the open end with the stopper, I observed,

that that part of the tube which I held in my hand was manifestly of a deeper colour than any other part of it. On this I held one end of it to the fire, and found that that end grew most intensely red, three or four times more so than the rest of the tube. The direction in which the tube was held made no difference with respect to the red part of it; the part that was hottest being always of the deepest colour, whether it was held upwards or downwards; so that whether the heated vapour ascended or descended, it did not retain its colour in the smallest degree, after it had been opposite to the heated part of the glass.

That this extraordinary redness was not occasioned by the vapour being more *rarefied* in that particular place, appeared by the whole tube assuming the same deep red colour, when the whole length of it was made equally hot: for the vapour being closely confined, the density of it within the tube must necessarily have continued the same in all the variations of heat or cold. This redness, therefore, must be the proper effect of *heat* on the phlogiston, as I should imagine, of the vapour. Repeating this experiment very often, with the same tube, and the same vapour, it became alternately of a deeper or lighter colour, according as it was kept hot or cold, without any sensible change, except that which depended upon this single circumstance,

cumstance. This is really a striking experiment, and especially when the tube contains just so much vapour as to be nearly transparent when it is cold; so that the heat alone gives it all the colour that it acquires.

In order to observe the *utmost effect of heat* on this vapour, I placed the closed end of the tube near the fire, and bringing it gradually nearer and nearer, observed that the colour deepened uniformly with the increase of heat, till, the glass actually melting, the confined vapour burst its way out.

* It seems probable from other phenomena, that if this vapour was not confined, but had room to expand itself, it would become colourless with heat. This, at least, is the case when it is combined with water. The phenomena I refer to are very common in the process for making dephlogisticated air, in which I first observed them. But the same things are observable in the processes for producing any other kind of air, in which much spirit of nitre is made use of; and likewise, constantly, in the common process for making spirit of nitre itself. It is, that when the heat is moderate, the vapour within the glass tube or retort is red, but that, as the heat increases, it becomes transparent.

In making dephlogisticated air, I have frequently observed, that, for a long time together, the tube

through which the air was transmitted was quite transparent next to the fire; but that near the water, where the air was delivered, it has been quite red, and even within the phial itself, after it had been transmitted through the water. It is also constantly observed, in the process for making spirit of nitre, that red fumes first appear in the retort, then in the adopter, if any be used, and lastly in the receiver; that when the heat is greatest, the retort becomes transparent, while the adopter and receiver continue red; and that when the heat is very great, the adopter will become transparent, and the receiver only will be red. I have also observed that as the heat intermits, in the course of a process, the redness returns into the adopter and receiver, and is constantly driven back again as the heat increases. I have likewise frequently observed that when there has been nothing in the retort but *colourless vapours*, like those of water, yet that, when they have passed into the receiver, they have immediately appeared in the form of red clouds.

It is remarkable, however, that, in the last stage of the process for making spirit of nitre, the red vapour always re-appears in the retort, which, at last, becomes most intensely red, so as to appear almost black; and this will be the case though the heat be increased ever so much. It is probable, therefore,

therefore, that heat has this power of attenuating the nitrous vapour, and making it colourless, only when it is combined with water, as well as with phlogiston; so that it disappears, as it were, and is concealed in the *steam*; but that after the distillation is over, and there is no moisture left for the acid vapour to combine with, that, in this case, as well as in that of my long tube, heat only contributes to make the vapour more red.

That this redness of the nitrous vapour disappears when the phlogiston is combined with *air*, as well as with *water*, was evident from several of the observations that I have made. I was led to ascertain this circumstance more particularly, by having a small phial filled with this red vapour, which I generally carried about with me, in order to shew it to my friends; when I observed that, in the space of about a month, though the phial was well closed with a ground stopper, as sufficiently appeared afterwards, the colour gradually disappeared; so that at last the redness was barely distinguishable by the application of heat, and, of course, it would answer my purpose no longer.

Upon this I took it for granted, that the phial had not been well closed; but, in order to satisfy myself with respect to it, I opened the phial under water, when it was immediately half filled with it.

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Had I examined the air within the phial, it would certainly have been found to be phlogisticated. But though I neglected to do this at that time, I took sufficient care to ascertain that circumstance afterwards.

For presently after this I opened under water another phial, which had been filled with the red vapour about two months before, and the colour of which was evidently fainter than it had been, when the water immediately rushed in, and filled two thirds of it, and the air within it was not at all affected by nitrous air.

At another time I made a very dry and clean phial but slightly red with the nitrous vapour, and observed that this slight redness presently disappeared, so as not to be recovered even by the application of heat; which certainly proves that the phlogiston had quitted the nitrous acid, and had united itself to the air; and that this colour appears in its union with the former, but not with the latter.

Lastly, I found, in the course of these experiments, that the power of this red vapour to phlogisticate common air was much greater, and acted much quicker, than I had imagined when I made the first observation of the kind. For, after the former observations, I filled another phial with the red vapour, and immediately afterwards opened it under water; when the water, rushing in, filled
about

about half of it, and the remaining air was found completely phlogisticated, not being in the least affected by nitrous air.

These observations confirm those I have recited before, concerning the vapour of spirit of nitre injuring common air. But how much of the effect arises from this cause, and how much is to be attributed to the nitrous air, necessarily mixed with the nitrous vapour, is not easily ascertained.

When I wrote the above, I filled tubes and phials with the red nitrous vapour, by means of the rapid solution of bismuth in spirit of nitre, which is a troublesome operation, when the tube is to be sealed hermetically after being filled with the vapour. The manner in which I succeeded in this experiment would be tedious to describe, and it would be unnecessary, as I have since effected the same thing in a much easier manner. For red lead converted into a white substance (as I have observed it to be by impregnation with the nitrous vapour, and which may be kept in that state without deliquescing any length of time, and without seeming to be disposed to part with any of the vapour which it has imbibed in the temperature of the atmosphere) readily emits it in a melting heat. I therefore put a small quantity of this white minium into a glass tube closed at one end; then, holding it to the fire, made it emit the red vapour, till the whole

tube

tube is filled with it; and having the other end of the tube drawn out ready for closing, as soon as the vapour begins to issue out of that end, I apply my blow pipe and seal it.

By this means I conclude that the tube is filled with a pure red vapour, without that mixture of nitrous air, and perhaps common air also, which I could not exclude before; and when this is done, I can easily, afterwards, melt off that part of the tube which contains the minium, so that it does not at all appear in what manner the tube was filled with the vapour. A tube thus prepared will become of a deeper colour with heat, and paler with cold, exactly as the tubes filled in the manner described before. A little moisture is expelled from the white minium along with the red vapour, but it is very inconsiderable.

This white minium never fails to be produced when, in any circumstances, the common minium is sufficiently impregnated with nitrous vapour. In making a quantity of dephlogisticated air from the common minium and spirit of nitre, I once filled a whole gun barrel with the materials; and when I emptied it, after the process, in which the bottom of the gun barrel only had been affected with the heat, I found part of the minium, at a small distance from the place that had been the hottest, perfectly white, while that from which the air had been expelled was yellow,

yellow, as usual, and that which was farther from the heat than the white minium, was almost black.

Having had a slight suspicion that the whiteness of this minium might possibly have been occasioned by something from the *bismuth*, carried over along with the nitrous vapour produced in the solution of it, I made a similar process with the solution of *iron*, and found that it had the very same effect as the solution of bismuth, converting the minium into a white substance, exactly like that which I had procured before. It is, therefore, the pure effect of impregnation with nitrous vapour, but certainly a very extraordinary one, and it may be well worth while to extend the process to various other solid substances.

SEC.

SECTION XII.

Of the Influence of Light on Vapour of Spirit of Nitre.

LIGHT, besides serving the important purpose of *viston*, is likewise a chemical principle, the effects of which are as yet but little known; though we have seen sufficient reason to conclude, that it is a very important agent in the system of nature. Any new facts, therefore, relating to so curious a subject, must be acceptable to the natural philosopher.

After the experiments which I had before made on the colouring of spirit of nitre, by which it appeared to depend upon phlogiston, it was suggested to me by a philosophical friend, that the *air* incumbent upon the acid might possibly affect its colour, and I was desired to attend to that circumstance. Accordingly I did so, and found that air *as such* had no influence in the case, but a very material one, as affording space for the vapour of the acid to expand itself in; so as to be, in that state, subject to the action of *light*, a thing of which I had no suspicion before.

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Having made a quantity of colourless spirit of nitre, which is readily done by making it boil hastily (in order to prevent too great a loss of the acid) and letting it cool again in the dark, I put different portions of it into several phials, some of them quite full, and others only half full, with every different species of air incumbent upon them, except the nitrous; which I knew would immediately be decomposed, and give it colour. Then leaving the phials exposed to the light of the sun, in a few days I found the acid in all of them that were only *half full* considerably coloured; whereas, the acid in the phials that were quite full remained as colourless as water.

After having had air of different kinds in those phials which were only partially filled, I contrived to have a *vacuum* above the acid; but still, when it was exposed to the light, it became coloured, as well as that which had air in contact with it*.

I then took some of the phials that were only *half full*, and covering them from the light, exposed them for several days to a considerable degree of *heat*. But in that situation they never acquired any colour.

* In this experiment I used a glass transferrer, executed by Mr. Parker. It is of excellent use when either acids or mercury is employed.

Being

Being now satisfied that it was the action of *light* upon the *vapour* of spirit of nitre that gave it colour, I amused myself with throwing a strong light, by means of a lens, into the upper part of a phial, the lower part of which contained colourless spirit of nitre. And in this manner I found that I could soon give a strong orange colour to the vapour of the acid; and that, being imbibed by the liquid acid with which it was in contact, *this* also became coloured, first at the top, and then quite through its substance. Other experiments shew that nitrous acid becomes coloured by the expulsion of dephlogisticated air, which is effected by heat.

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SECTION XIII.

Of the Impregnation of Water with phlogisticated nitrous Vapour.

HAVING exactly measured a quantity of water before it was impregnated with the nitrous vapour, I observed that, after the impregnation, it was increased exactly one third; *two* measures of water having become *three*, and agreeably to this, I found that a quantity of the strongest spirit of nitre that I could procure, occupying the space of four pennyweights of water, weighed six pennyweights. The exhalation of red vapour from water thus impregnated is very great, far exceeding any thing that I have ever seen in any other kind of spirit of nitre. When the stoppers have been thrust very hard into the phials containing it, they have been sometimes thrown out with great force, and the upper part of the phials, containing this acid, are always exceedingly red.

Examining the strength of a quantity of deep green spirit of nitre, I found that as much of it as occupied the space of four pennyweights of water,

yielded thirteen ounce measures of nitrous air with copper. When a quantity of this acid had been exposed to the open air about a week, and its colour had intirely vanished, the same quantity of it yielded nine ounce measures of nitrous air, which is about the same produce that was yielded by a pure spirit of nitre mentioned above; and being equally *colourless*, they were probably the very same thing. N. B. No heat was applied in these trials.

The *volatility* of the nitrous acid, in water impregnated in this manner, is very extraordinary. For, pouring a little of it into an open glass, and blowing upon it, a copious red vapour issues from it; and by blowing upon it in this manner about the space of a minute, the blue or green colour intirely vanishes, and the water becomes of a pale yellow colour, exactly like common spirit of nitre. If the acid in this state (and the same is the case with common spirit of nitre) be exposed to the open air a few days, it becomes quite, or very nearly, colourless, and very weak. To see the nitrous acid thus blown out of a quantity of this impregnated water in the form of a red vapour, just as it went into it, is curious enough, and what has given much pleasure to those of my chemical friends to whom I have shewn it. It is evident from this experiment, that the thing on which all these colours of nitrous acid depends, is the volatile acid vapour.

Oil

Oil of vitriol is known to increase in weight by being exposed to the open air, from which it attracts a quantity of moisture, which dilutes it. But this is not the case with the nitrous acid; at least with this. For, on the contrary, it always loses both weight and bulk by such exposure. In order to observe the limit of this loss with respect to this volatile spirit of nitre, I observed that a quantity of it, before it was so exposed, weighed thirteen pennyweights eight grains; and after a fortnight, it had lost one pennyweight, and was diminished about one fifteenth in bulk.

I found, however, that the strongest impregnation of water with nitrous vapour, besides containing a quantity of acid more volatile than usual, retains as much of it as the strongest spirit of nitre does after being a long time equally exposed to the common air. And, indeed, when the green colour is blown out of this impregnated water, it is not to be distinguished, in any respect, from the strongest yellow spirit of nitre.

In order to observe in what *proportion* different kinds of nitrous acid would lose strength by exposure to the open air, I exposed in equal cups, equal quantities of *blue spirit, green, green tinged with yellow*, a quantity of the common sort made by Mr. Godfrey, and a quantity of my own distilling, with the proportion of eight ounces of oil of vitriol to ten of

nitre ; and when they had stood about a fortnight, and were all become quite colourless, like water, a quantity of each of them occupying the space of four pennyweights of water, yielded nitrous air in the following proportions. Of the green four ounce measures, and of the green tinged with yellow, four and three quarters ; which was also the produce of Mr. Godfrey's acid ; and of my own five ounce measures. Of the *blue* I find no account.

That the spirit of nitre made by the impregnation of water with nitrous vapour, is a purer acid of the kind than the common spirit of nitre, appears evidently by its not depositing any thing when it is mixed with a solution of silver in the nitrous acid, which is the case with the common spirit of nitre ; and this is always said to be a proof that it contains a quantity of vitriolic acid.

But that spirit of nitre made in this manner contains more phlogiston than common spirit of nitre is also evident, both from the copious red fumes emitted from it, and also from other circumstances, especially from the *quality of the air* which it yields with flowers of zinc.

Having mixed a quantity of blue spirit of nitre with flowers of zinc which were of a dull colour, and appeared from several experiments to contain a portion of phlogiston, it yielded, with the heat of a candle applied to the phial which contained it,
strong

strong nitrous air ; when the common spirit of nitre applied in the same manner gave only phlogistated air ; the phlogiston of which came probably from the calx itself, though a small portion of it might have been in the nitrous acid, which I believe is never intirely free from it.

It is also a proof that the green spirit of nitre contains a good deal of phlogiston ; that if a very brown smoking spirit of nitre, which certainly contains much phlogiston, be mixed with water, in a certain proportion, a green acid is produced. But this is not the case when the common yellow spirit of nitre, which contains less phlogiston, is applied in the same manner.

A most remarkable fact of this kind once occurred to me, in the course of my distillation of spirit of nitre. The saltpetre being very impure (having been casually mixed with various phlogistic matters) the acid that came over was exceedingly brown, and the fumes were uncommonly red, and copious ; when, the fire happening to slacken, there was a condensation of air and vapour within the vessels, and a quantity of water rushed, before I was aware of it, into the receiver, through the glass tube, *f*, fig. 4. Pl. V. and this mixture of that strong brown spirit of nitre and the water, which I suppose was three fourths of the whole, made a perfectly

deep green acid ; whereas none that I could make by the mixture of the acids after they were decanted, ever approached to the greenness of this. The colour was almost as deep as I could produce by the direct impregnation of water with the nitrous vapour.

I have observed that the consequence of impregnating water with the vapour that escapes from spirit of nitre is making it sparkle, with the spontaneous production of nitrous air. This seems to prove that, unless there be earth in all water, there cannot be any earth necessarily contained in nitrous air. But at that time I had always procured this appearance by throwing into the water the red nitrous vapour from a violent effervescence of spirit of nitre and bismuth ; and in this violent effervescence it was possible that some of the earth of the metal might be carried over, as some of the water evidently was. I was, therefore, now careful to avoid this objection, which I did by exposing a phial of pure nitrous acid to nitrous air over the purest distilled water. This I did by means of a tube with a ground stopper at each end. For by stopping and unstopping them alternately, I could easily manage so as to place the phial of spirit of nitre, supported by a thin glass tube, very near the top of the vessel, then fill it quite to the edge of the vessel with water, and

and after that displace the water by introducing nitrous air. As the nitrous air was absorbed I introduced more, by means of a bladder previously filled with it. The quantity of common air above the spirit of nitre was quite trifling in proportion to the bulk of the tube.

In these circumstances I observed that when the nitrous acid became blue, and hardly before, the water next to it began to emit bubbles of air. To the formation of this air (which was doubtless nitrous air) nothing could contribute but the effluvia of the nitrous acid, and something that the water itself might furnish; and this water had been slowly and carefully distilled in glass vessels.

Of this immediately became a very dense
by this expansion, and as it was highly
After standing all night it became yellow
and separated; and being divided by heat it still
retained its yellow colour; but by a fresh im-
pulsion it became blue, and after that of a light
orange.

After standing some weeks it became white
and stiff; but was a little fluid at the bottom
of the phial, where it also inclined to a green co-
lour, while the upper part had something of the ap-
pearance of oil, as if it had been of an acid nature
connected with it, and were changed in it.

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SECTION XIV.

*Of the Impregnation of Oils, and of Spirit of Wine,
with the nitrous Vapour.*

THE effect of the impregnation of *oils* with the nitrous vapour is, in general, the coagulation of them, and giving them a red colour. But the phenomena attending the processes, from the first to the last stage of the impregnation, are very various, and not a little remarkable.

Oil of *olives* immediately became of a sky-blue colour by this impregnation, and at first was slightly warm. After standing all night, it became yellow, and coagulated; and being dissolved by heat, it still retained its yellow colour; but by a fresh impregnation it became blue, and after that of a light orange.

After standing some weeks, it became almost white and stiff; but was a little fluid at the bottom of the phial, where it also inclined to a green colour, while the upper part had something of the appearance of *foth*, as if small bubbles of air had been emitted by it, and were entangled in it.

A part

A part of the *whale oil* that had been used in my first attempts to confine the nitrous acid vapour, and which was of a dark red colour, and coagulated, being melted, and put into another phial, and then impregnated again with fresh nitrous vapour, became of a deep blue colour. In cooling it became of a dirty green, then inclined to yellow, became a little fluid, and continued so.

A quantity of *fresh whale oil* became blue by this mode of impregnation. Leaving it to cool, the upper part of it became stiff, and assumed a light orange colour, while the lower part still continued blue and fluid; but at length it became of a deep orange colour, and was perfectly coagulated throughout. By long keeping, the lower part of it became fluid, while the upper part continued stiff, the whole of it looking brown; but the fluid part became of a lighter colour than the other.

Oil of *turpentine* presently became of a thick consistence, and yellowish. Repeating the process some time after, all the upper part of the phial was filled with dense white fumes, and the lower part of it became quite red and stiff. At one time, during the process, when the stream of nitrous vapour was peculiarly copious, there was a kind of slight *explosion* in the inside of the phial. This substance always continued very stiff and red.

Having

Having put a little *essential oil of mint* into a phial previously filled with nitrous vapour, a violent effervescence, and great heat, were immediately excited; while the oil presently became of a green colour, and the smell of it was exceedingly strong. Afterwards, by throwing more nitrous vapour into the phial, this oil became of a deep orange colour, was hardly fluid, but was semi-transparent. By long keeping, however, it became almost solid, like the oil of turpentine, but of a brighter colour; and so it still continues.

When these kinds of oil, about two months after they were impregnated, were melted by the heat of the fire, they all retained the same colour. And at this time I observed that the solid part of the whale oil swam in the melted part, occasioned perhaps by its having some bubbles of air entangled in it, though none of them were visible. Each of these kinds of oil still preserved their peculiar smell, though mixed with that of the spirit of nitre.

Ether received no change of colour at first by this impregnation; but the upper part of the phial was filled with a white fume, when the vapour was first applied; and when a candle was presented to the mouth of it, it burned with a green flame, exactly like a mixture of inflammable and nitrous air. At length all the upper part of the fluid became
of

of a deep blue colour; and, observing it more narrowly, the blue appeared to be entirely separated from the rest, which continued colourless at the bottom, and was three fourths of the whole.

Reflecting upon the phlogisticated air emitted from the whale oil, after being impregnated with nitrous vapour, in my attempts to confine that vapour by means of it, I was willing to try whether a similar impregnation of the other oils, as *linseed oil*, and *oil of turpentine* would produce the same effect; which I found it to have. It was in this course of experiments, that I accidentally observed the astonishing effect of these oils to decompose *nitrous air*, and leave it in the state of phlogisticated air, of which an account has been given already.

The oil of *turpentine* took much more of the nitrous vapour than the *linseed oil*; but when, in the course of the process, it became blue, as the other had also done, it yielded air more copiously; and this, like the air from the whale oil, and the *linseed oil*, was mere phlogisticated air, without any mixture of fixed air.

Not having a quantity of *ether* to make the experiment with this species of oil, in the same manner as I had done with the others, I contented myself with impregnating it by plunging the end of the tube, out of which the vapour issued, into
a phial

a phial containing the ether. This I did a considerable time without any particular appearance; but at length the ether suddenly turned green, and the moment that this change of colour appeared, air began to issue from it in torrents.

Happening to make the effervescence too violent, at one time that I was making this experiment, some of the nitrous acid, and small bits of the bismuth, were thrown into the ether; where they kept dissolving, and producing air very copiously. This air, when I first observed it, I concluded to be nitrous air; but when I collected a quantity of it, and examined it, it appeared to be phlogisticated air only: from which it was evident, that ether had the property of instantly converting nitrous air into phlogisticated air, or at least of yielding phlogisticated air from the impregnation of nitrous vapour.

Suspecting at that time that the former of these was the case, and willing to try whether the other kinds of oil had the same property, I transferred a quantity of nitrous air into a phial previously filled with oil of turpentine; and observed, that, with a very small degree of agitation, it was absorbed very fast; and, being reduced to one fourth of its bulk, was found to be mere phlogisticated air. This experiment suggested those that have already been recited, concerning the effect of oil of turpentine upon

upon nitrous air. N. B. The impregnation of ether with nitrous vapour seems to make it more volatile than it was before.

The effect of the impregnation of *spirit of wine* with the nitrous vapour was considerably different from that of the oils above-mentioned. When I threw a stream of nitrous vapour upon a quantity of this fluid contained in a phial, it did not suffer any change of colour, and was not sensibly heated; but when the flame of a candle was presented to the mouth of the phial afterwards, a vapour issued from it, which burned with a greenish flame.

After frequently repeating this process, a quantity of genuine *nitrous ether*, about one third of the whole, was perceived to separate itself from the spirit of wine, and to swim upon the surface of it.

Both the ether and the spirit of wine were rendered extremely volatile by this process, the vapour frequently throwing out the glass stoppers from the phials in which they were contained, if they were not thrust in very tight; and bubbles of air issued very copiously from every part of them, whenever the stoppers were taken out, a quantity of the vapour always rushing, at the same time, out of the phial.

When

When I made this impregnation by plunging the tube out of which the vapour issued into a phial of this liquor, the process was continued a considerable time before any thing remarkable appeared; but at length the liquor turned suddenly blue, and boiled with great violence; when, immediately inverting the phial, and filling it up with fresh spirit of wine (in order to place it inverted in a basin of the same) all the vapour was again absorbed, except a very small bubble. But, by the help of a little warmth, more air was produced, and it expelled a great part of the liquor. This air being transferred to quicksilver did not affect it; and upon admitting lime water to it, though one half of it was presently absorbed, the water did not become turbid. Applying the flame of a candle to the mouth of the vessel in which it was contained, it burned with a blue flame descending pretty rapidly from the top to the bottom of the vessel.

Upon this I took a quantity of spirit of wine partly impregnated with nitrous vapour, about two months before, and warming it, found that it yielded air, or vapour, in the same manner as in the last experiment; and filling a phial with this liquor, and inverting it in a basin of the same, the vapour issuing from it presently filled almost the whole of the phial. Transferring this air into a vessel of

lime water, three fourths of it presently disappeared, the lime water was made considerably turbid, and the residuum seemed to be slightly inflammable.

At another time I observed that air produced in this manner was phlogisticated air, mixed with fixed air, and also with the vapour of ether, which I have found to double the quantity of any kind of air. That fixed air may come from a mixture of spirit of wine and nitrous acid, I had observed before, and this seems to be a pretty satisfactory proof, or at least a strong presumption, of fixed air being a modification of the nitrous acid. For by no other treatment, I believe, can spirit of wine be made to give any sign of its containing fixed air.

N. B. It may be just worth while to observe, at the close of this section, that *whale oil* and *olive oil*, impregnated with nitrous vapour, dissolved quicksilver, and produced air; but the oil of *turpentine* impregnated in the same manner, did not seem to affect quicksilver. I made these trials by only dropping a little quicksilver into the impregnated oils, and observing whether any air bubbles rose from it.

SECTION XV.

Of the Impregnation of the Acids, &c. with the nitrous Vapour.

BOTH *oil of vitriol*, and *spirit of salt*, receive an impregnation from nitrous vapour, though not in so great a quantity as water; and the effects of these impregnations are pretty remarkable, and in several respects considerably different from the result of a mixture of these acids, when each of them (as hitherto they always have been) are previously combined with water.

Having filled a very large phial with the nitrous vapour, I poured into it a little *oil of vitriol*; and observed that the vapour was imbibed, though very slowly; but at length the red colour entirely disappeared, and the air rushed into the phial when it was opened. Afterwards I fully saturated a quantity of strong oil of vitriol with the red vapour thrown upon it, in a large phial, frequently shaking it, to promote the saturation, and repeating the process very frequently. At length this acid, from being quite transparent, became of a light blue
I. colour;

colour; but in other respects did not differ much, to appearance, from common oil of vitriol, except that a white vapour exhaled from it.

But having poured a quantity of this impregnated oil of vitriol from one phial into another, and having dipped the empty phial into a trough of water, in order to rinse it, I was surprized by the sudden bursting out of a great quantity of *red vapour*, which dashed part of the water to a considerable distance, and also at a prodigious *heat*, that was instantly produced within the phial.

Upon this I put a small quantity of the impregnated oil of vitriol into an open drinking glass, and gently pouring a little water upon it, observed no remarkable appearance; till, with a piece of glass tube, I began to stir and mix them; when the heat immediately took place, attended with the emission of a cloud of dense red vapour: and these appearances increased with the agitation, and intimate mixture of the water and this impregnated oil of vitriol, till at length all the nitrous vapour seemed to be expelled, and the oil of vitriol was left as it was at first, only diluted with water. This experiment, when well conducted, is more remarkable than it will be expected to be found from this account of it. Several good chemists of my acquaintance have been much struck with it.

I called this *nitro-aqueous acid*.

But the impregnation of *spirit of salt* with the nitrous vapour, at the same time that it is more easily effected, is of a still more remarkable nature; and will, I hope, be of considerable use: for it makes an *aqua regia* of incomparably greater power in the solution of gold than the common sort. In consequence of this impregnation, the spirit of salt, from being of a straw colour, presently becomes of a very deep orange, much deeper than the spirit of nitre itself can ever be made, and the vapour which it emits is peculiarly pungent.

But one of the most remarkable circumstances attending this impregnation is that, whereas the common *aqua regia* is made in the best manner by mixing one fourth of the spirit of salt, with three fourths of spirit of nitre, this liquor, which is, in a manner, nothing but spirit of salt (for I did not perceive that the bulk of it was sensibly increased by the process) after having imbibed a little of the nitrous vapour, becomes possessed of all the properties of that *aqua regia*, which consists chiefly of spirit of nitre, and in much greater perfection. For, while the common *aqua regia* will hardly dissolve gold without the help of *heat*, this dissolves it with as much rapidity as I have almost ever seen in any chemical solution whatever, when it is perfectly cold. The quantity of gold that this *aqua regia* is capable of dissolving, in proportion to its bulk, I

have

have never ascertained; but it seems to exceed the common aqua regia very much in this respect.

This is also a much *cheaper* kind of aqua regia than the common sort. For a small quantity of spirit of nitre will, by this means, communicate a sufficient quantity of nitrous vapour to saturate a large quantity of spirit of salt, which is a very cheap article; whereas, in the common method of making aqua regia, the bulk of it is spirit of nitre, which in comparison is dear.

If nitrous air be decomposed over a quantity of spirit of salt, in the manner described in a former section, it becomes the same powerful aqua regia, by saturation with the nitrous vapour, which had been contained in the nitrous air.

I thought it something remarkable, that after having made aqua regia by impregnating spirit of salt with nitrous vapour, I could not compose an aqua regia by impregnating spirit of nitre with the vapour of spirit of salt, applied in the same manner. But though I endeavoured to do this in every method that I could think of, I got no liquor that would dissolve gold, or that was, in any respect, materially different from common spirit of nitre.

When spirit of salt is saturated with nitrous vapour, it yields air, in the same manner that water

does in the same process. This air I caught, and found it to be nitrous air, the same that it yielded by water. Part of the air, however, was absorbed by water; which I imagine must have been a mixture of marine acid air, discharged from it together with the nitrous air.

Water impregnated to saturation with vitriolic acid air admits of this impregnation with nitrous vapour almost as well as pure water. In this process, which was performed by plunging the tube from which the vapour issued into a phial of this impregnated water, standing in a basin of cold water (in order to prevent its becoming hot, and thereby losing its own proper air) it became blue, and emitted air very copiously. Much more than the bulk of the water had escaped, when, by filling a phial with this doubly-impregnated water, and inverting it in a basin of the same, I caught a quantity of it, and found it to be pure nitrous air. There would, no doubt, have been a mixture of vitriolic acid air along with it; but water presently imbibes this kind of air. In making this impregnation, the greatest care should be taken to make it retain the vapour which the water has imbibed, by always keeping the phials which contain it closely stopped, and also by plunging them in cold water as soon as the operation is over. For otherwise the effort

effort of these two very elastic vapours to escape from the water would endanger the bursting of the phials.

This water impregnated with vitriolic acid air, and super-impregnated with nitrous vapour, emits a copious *white fume*, in which, as well as in its blue colour, it resembles oil of vitriol impregnated in the same manner. But I could not make oil of vitriol emit any nitrous air by this process.

Having made these new impregnations, I was desirous of observing the effects of them in the *solution of metals*; and it was observable in general, that, in these processes, as well as in the experiments with the mixture of acids, the nitrous acid produced its effect the first, or at least in the greatest abundance at first; while the other acids seemed to act their parts independently of it, taking more time to their work; but several of the phenomena were singular enough.

The oil of vitriol thus impregnated readily dissolved quicksilver, but without yielding any air at first. But when the whole seemed to be dissolved into a thick and white matter, air was produced pretty plentifully, though irregularly, and it was all nitrous. The upper part of the liquor in the phial in which this solution was made was green, and the lower part white. This oil of vitriol also dissolved silver, but would not touch gold.

The spirit of salt which had received this impregnation, and which dissolved gold so rapidly, as I have mentioned, dissolved *silver* also, and produced nitrous air. This liquor also dissolved *zinc*, and produced air that was strongly inflammable, differing in nothing from common inflammable air, except that it burned with a green flame, which must have been derived from a slight mixture of nitrous air.

Water saturated with vitriolic acid air, and then with nitrous vapour, would not dissolve gold; but, though much diluted with water, it yielded air from zinc, one measure of which and two of common air occupied the space of two measures and a half. The air thus produced without heat extinguished a candle; but when I heated the phial, I got more air, which was fired with one great explosion, like a mixture of inflammable and common air; or rather like inflammable air fired in the vapour of spirit of nitre, the flame descending to the bottom of the phial, and being exceedingly bright.

Repeating the experiment, in order to take the air at different times, I found that the first produce, without heat, after passing as little as possible through water, burned with an enlarged flame, a bright flame in the center, and a blue one at the surface. Afterwards the air was wholly inflammable, a clear
bright

bright flame descending rapidly from the top to the bottom of the phial; and the last that I could procure exactly resembled a mixture of inflammable and nitrous air, burning with a green or yellowish flame in the mouth of the phial, and at length descending gently from the top to the bottom.

At another time this doubly-impregnated water, with zinc, produced strong nitrous air, afterwards that which burned with an enlarged flame, then with a flame still more enlarged, then like a mixture of common and inflammable air, the flame descending at once to the bottom of the vessel; and the last produce was fired at several times, exactly like a very weak inflammable air.

But none of the appearances recited above are so remarkable as the phenomena produced by the vapour which I procured on distilling to dryness a solution of gold in marine acid impregnated with nitrous vapour, which I have mentioned as so excellent a kind of *aqua regia*. The produce of this process was an *acid air*, of a very peculiar kind; partaking both of the nitrous and marine acid, but more of the latter than of the former, as it extinguished a candle; but it was both extinguished and lighted again with a most beautiful deep blue flame. A candle dipped into the same jar of this kind of air, went out more than twenty times successively,

making a very pleasing experiment. The quantity of this acid air is very great, and the residuum I have sometimes found to be dephlogisticated; sometimes phlogisticated, and at other times nitrous air.

When I endeavoured to receive this vapour in quicksilver, instead of water, the first that I took corroded the quicksilver very much. Having completely filled a pretty large jar with the first produce of this air, the quicksilver presently rose within one fourth of the top, which I took for granted had been by the solution of mercury in the nitrous acid; and therefore I expected to find this air nitrous. But when I examined it, it appeared to be mere phlogisticated air. There had probably been a production of dephlogisticated air, which had been reduced to phlogisticated air by a mixture of nitrous air produced by the solution of the quicksilver. After this, in the same process, I filled another jar half full of this air, attended, as before, with a great corrosion of the quicksilver; but it rose no higher, as it had done in the preceding experiment; and when water was admitted to it, the whole of it was instantly absorbed.

It was probably the nitrous vapour that came first in this process, and the marine acid air afterwards.

It

It is easy to catch this curious vapour by attending to the course of the process, which is as follows. The first thing that comes over is the common air contained in the phial (for I always made the experiment in a glass vessel and a sand heat) then the fluid begins to distil; immediately after which comes this pure acid vapour; and in the last place the generated air, the quality of which is various, but generally dephlogisticated.

It may be just worth while, at the close of this section, to mention the impregnation of a solution of liver of sulphur in water, and also of alkaline liquors, with this vapour.

Throwing the nitrous vapour upon water saturated with liver of sulphur, it presently became milk white; but growing clear, it became of a light colour, a substance in the form of curds swimming at the top of it, and which was probably the same matter that had made it appear so white and cloudy at first.

Throwing this vapour upon the *volatile sal-ammoniac* a white cloud was immediately produced within the phial, and it continued a long time; but at length it disappeared, and the liquor became first of a slight orange-colour, and after some time was blue. In this process the liquor became very hot, and small bubbles of air issued from it in great plenty. These, had

had I examined them, would probably have been found to have been nitrous air.

I began the same process with *caustic alkali*, and observed that it imbibed a great quantity of the nitrous vapour; but seeing nothing remarkable in the appearance of the liquor, I did not prosecute the experiment.

The phosphoric acid is presently saturated with nitrous vapour, and assumes a deep indigo blue colour.

Radical vinegar is also soon saturated with this vapour, and assumes a light blue.

Spirit of salt saturated with fresh minium, so as to be of a yellow colour, becomes of a deep orange when impregnated with nitrous vapour.

Spirit of salt saturated with white minium, made so in consequence of the colour being extracted from it by the spirit of salt, assumes a light blue colour by being impregnated with this vapour.

Spirit of salt saturated with red precipitate, or the precipitate *per se*, assumes a green colour.

Spirit of salt saturated with flowers of zinc acquires a blue colour, deeper than a sky blue, but not so dark as the blue of the phosphoric acid.

As a mixture of the nitrous and marine acid makes *aqua regia*, which dissolves gold, I had thought it might be possible, that common spirit of salt, after dissolving some of the nitrated calces
above

above mentioned, might have the same property; but it had not. It is now pretty well confirmed, that it is the marine acid alone, in the composition of aqua regia, that dissolves the gold; this acid being dephlogisticated by the spirit of nitre, which has a stronger affinity with phlogiston than the marine acid has.

Precipitate *per se* dissolves with great rapidity in spirit of salt. A quantity of this solution I impregnated with nitrous vapour, on which the surface of the fluid, and the sides of the phial, were instantly covered with crystals, larger than those of the precipitate. The colour of the liquor was then of a light blue, or green. Afterwards it assumed a deep brown, inclining to yellow, and a great quantity of white matter was deposited, occupying almost the whole space of the liquor. This must, I presume, have been corrosive sublimate, the pure air from the precipitate having dephlogisticated the spirit of salt, which is a necessary circumstance in this preparation,

SECTION XVI.

Of Crystals formed by the Impregnation of Oil of Vitriol with phlogisticated nitrous Vapour.

I Have already observed the remarkable effects of impregnating oil of vitriol with nitrous acid vapour.

Having impregnated a larger quantity of the oil of vitriol than I made use of in those experiments, I left some of it in a large phial, with a ground stopper, among other phials containing things for which I had no immediate use. But though my process was over, that of nature was not. Happening to be looking at it on the 19th of March following, perhaps about six months after the impregnation, I found what I was far from having expected, viz. that almost the whole was crySTALLIZED, a very small part only of the contents of the phial remaining liquid. The crystals looked exactly like ice, and exhibited all the appearances that I had before observed to attend the simple impregnation of the vitriolic acid with nitrous vapour, but in a much more elegant manner. For on dropping a piece of this

ice into pure water, it became green, and effervesced with great violence; and, what made a beautiful and striking phenomenon, all the water in which the ice was dissolved began instantly to sparkle, with the spontaneous and copious production of air. With the help of a little heat, this production of air was so great, that the quantity was more than a hundred times the bulk of the ice that had been dissolved. It was the purest nitrous air. In fact, a great quantity of nitrous vapour was, as it were, imprisoned in this oil of vitriol, and being suddenly set loose, on being plunged in the water, it impregnated the water in the same manner as I have observed that the nitrous vapour never fails to do.

The application of heat made this ice emit a dense red fume; but holding a quantity of it in a glass vessel over a candle, it presently melted, emitting bubbles; and then, letting it stand to cool gradually, it crystallized very suddenly, when it was about blood warm. It was in this second congelation much more opaque, and denser than it had been in the former. When this ice was dissolving with heat, the fume it emitted was not red, but white, and exceedingly dense, like oil of vitriol in vapour. After it had been kept dissolved, and in a boiling heat, some time, it did not crystallize afterwards,
but

but continued fluid and transparent; being then, probably, mere oil of vitriol.

I have not yet been able to investigate all the circumstances necessary to this remarkable crystallization, having originally found it when I had no expectation of any such thing, and having often failed to find it when I have expected it the most. All that I can do, therefore, is to recite what I have observed, with all the circumstances that I can recollect relating to the appearances.

I had kept about half an ounce measure of oil of vitriol, not quite saturated with nitrous vapour, in a small phial, with a ground stopper, about a year, in all which time it had shewed no tendency to crystallization, and from its imperfect impregnation I had not expected it. I was intending to complete the impregnation, and, looking at the phial, had taken out the stopper, and put it in again, deferring the process till the day following, when I found the phial almost filled with the most beautiful crystallizations imaginable.

Their form, as nearly as I can describe it, was that of a feather. They were about twenty in number, some of them as large as the phial could contain, and many of them parallel to each other, but others lying in different directions. The two parts, as it were, of the feather made an angle with each other

other of about 160 degrees, and each of the single fibres that composed the feather, but which were connected, like the toes of a duck's foot, by the same substance (but thinner, and more transparent than the rest) made an angle with the stem from which they arose of about forty five degrees. A more beautiful appearance can hardly be imagined, and I am afraid I shall never see the like again.

Having observed these crystals some days, and seeing no farther change in them, or in the liquor which covered them, and which rose about a quarter of an inch above them, I poured the liquor from the crystals, and for some time they continued upright, exhaling a red vapour, which filled the phial, and at length very much clouded and obscured it. This liquor exactly resembled strong smoking spirit of nitre, and seemed to have nothing of the vitriolic acid in it.

After some time the crystals seemed to decay, and sunk down in the phial, filling up all the interstices that had been among them, so as to make one compact mass, without any thing of the beautiful appearance that they made before. Hoping to repair the injury they had sustained, and to restore their beauty, I filled up the phial with fresh oil of vitriol strongly impregnated with nitrous vapour, but it had no sensible effect, nor did any more crystals of the

same, or of any other form, shoot out from them in many months.

Having another phial of oil of vitriol partly impregnated with nitrous vapour, and of about the same standing with the former, I examined it, and found it half filled with crystals, but these lay all confusedly at the bottom of the phial, and though in separate pieces, of no uniform shape.

After this I impregnated three different quantities of oil of vitriol with nitrous vapour. One was very strongly concentrated, having distilled off about half the quantity of the best common sort, the second was both distilled and concentrated, and the third was only of a medium strength, and the common sort, but colourless. I kept all these in the same situation, and in about a fortnight that which had been simply concentrated began to crystallize, and in about a fortnight more the phial was half filled with crystals, some of them in the form of feathers, but lying in different directions, and not detached from each other, but forming a compact mass.

In this state I left them, being obliged to be absent from my laboratory about three months; and at my return I found all the phials full of crystals, but generally in solid masses, with few such feathers as I have described above, and those very short ones.

Imagining

Imagining that this singular crystallization might possibly be accelerated by exposing the impregnated vitriolic acid to heat, I took a quantity of it which had continued a considerable time without crystallizing, and confined it in a glass tube three feet long, and half an inch in diameter. Then holding it to the fire, I observed that the acid emitted red vapour, which filled the whole tube, exactly as would have been the case with spirit of nitre itself. When it was cold many small crystals were scattered all over the tube above the surface of the liquor, and the upper part of it was red; being, I suppose, the spirit of nitre that had been driven out of it by the heat, as being more volatile than the vitriolic acid.

I have already observed that, to appearance, the vitriolic acid impregnated with nitrous vapour, was nothing but nitrous acid, after the complete formation of the crystals, and by experiment I found it to be so. For diluting it with water, and dissolving iron in it, in a phial with a ground stopper and tube, in the manner in which I usually produce nitrous air, it yielded this kind of air only, without any mixture of inflammable air; which I have formerly observed is the case when the vitriolic and nitrous acids are mixed together, and employed in the solution of iron, the nitrous air coming first, and the inflammable air afterwards.

Here, indeed, a very small quantity of the last produce burned with a lambent flame; but this I have observed to be the case with the last produce from iron and the nitrous acid only, when the process was urged, as it was now, with the flame of a candle. The water, when this acid was mixed with it, sparkled very much, yielding, I doubt not, nitrous air. But this circumstance only proves it to have been highly charged with phlogisticated nitrous vapour.

Here then is a case in which the nitrous acid appears to have a stronger affinity with water than the vitriolic; for in a course of time, it intirely expels the vitriolic acid from it, and unites with it itself; all the vitriolic acid being precipitated in the crystals that consist of both the acids.

Crystals similar to these may be produced at pleasure, if the vitriolic acid be highly concentrated, and the nitrous vapour very copious; but they will appear on the sides of the phial, and not in the body of the acid itself.

When the vitriolic acid is nearly saturated with the nitrous vapour, hold the phial (which should be a large one, containing about a quart) and turn it so as to moisten all the inside of it. Then immediately throw in a very copious nitrous vapour, so that the whole phial shall be intensely red, and running over; after which put in the stopper, and let it remain quite still.

still. The upper part of the oil of vitriol will then be of an orange colour, and all the sides of the phial, and especially the parts towards the bottom, will soon be quite covered with those crystals, but of different sizes. By degrees they will be formed on the surface of the acid; but in a few hours afterwards, when the nitrous vapour is equally distributed through the body of the oil of vitriol, all these crystals will disappear.

By repeating this process, one half of the whole body of vitriolic acid will be crystallized in an irregular manner, as if it was congealed. When I have poured the whole of this semi-congealed mass into a smaller phial, just large enough to contain it, the coagulated part has subsided to the bottom, and other crystals have gradually formed, shooting with some regularity from it into the middle of the superincumbent liquid, which has always become more pellucid, and approached more to the colour of spirit of nitre, in proportion as the crystals have extended themselves.

Finding that all the acid of vitriol was contained in the crystals, and that the superincumbent liquid became in time pure spirit of nitre, I was desirous of knowing whether, if there should be any phlogistic matter previously contained in the oil of vitriol, the phlogiston would be retained in the crystals, or pass into the spirit of nitre.

With this view I dissolved a small quantity of bees-wax in highly concentrated oil of vitriol, making it thoroughly black, and greatly increasing its viscosity; and afterwards I impregnated it with nitrous vapour, and shut it close up in a small phial. After some weeks the crystals began to form, and they were intirely white, just as if the vitriolic acid had been pure. The process is not yet completed; but I expect that the nitrous acid will be highly phlogistified. Does not this experiment seem to prove, that the nitrous acid has a stronger affinity with phlogiston than the vitriolic? The fact is certainly a pretty remarkable one.

SEC.

SECTION XVII.

Of the Action of Nitrous Vapour upon some solid Substances.

WITH respect to the articles mentioned in the title of this section, I have not done much; but some of the observations that I have made will be found to be curious.

Considering the extraordinary strength of the marine acid vapour, I was desirous of trying whether the nitrous acid in the same form would have the same power, viz. that of decomposing substances into which the vitriolic acid entered; and I made the experiments upon *sulphur*, and *alum*. But it did not appear that the vitriolic acid, in either of these cases, was dislodged by the nitrous; owing, perhaps, to the nitrous acid, in this case, being partially saturated with phlogiston, though uncombined with water. The sulphur was unchanged, but the alum was rendered white and opaque; an effect which I have observed to be produced by alkaline air; the acid in this case, as the alkali in that, hav-

ing seized upon the water that is contained in this saline substance.

Common *salt* imbibed this nitrous vapour ; but whether its acid was dislodged, or its water only was seized upon, I did not examine.

As spirit of nitre mixed with earth yields dephlogisticated air, I was willing to try whether the nitrous vapour, without water, would have the same effect ; and I made the trial with *flowers of zinc*, and *red lead* ; the former being of a darkish colour, and containing, I believe, more phlogiston than the whiter flowers of zinc.

Having frequently thrown a stream of this vapour upon a quantity of these *flowers of zinc*, I put them into a gun barrel ; and from one ounce, one penny-weight, six grains, weighed after the saturation (for I had neglected to do it before) I expelled only six or eight ounce measures of air, half of which was fixed air, and the remainder phlogisticated ; owing, perhaps, to the gun barrel, but perhaps also, in part, to the phlogiston contained in this calx. The materials carefully collected afterwards weighed one ounce seventeen grains.

But the effect of this vapour upon *red lead* is exceedingly remarkable. Common spirit of nitre mixed with this substance makes it of a deeper red, till at last it is almost black ; but the nitrous vapour,
after

after deepening the colour of it a little, changes it into a perfectly *white and brittle substance*, at the same time heating it exceedingly.

To produce this change in the red lead, I found, after many trials, that I succeeded best by first slightly moistening the inside of a glass jar, and, by applying the red lead to every part of it, giving the jar as thick a coating of it as I could; and after this throwing the vapour into it, by inserting the tube through which it issued very deep into the jar. By this means there was exposed to the nitrous vapour surface enough to imbibe it all, without suffering any part of it to go over the mouth of the jar. It is remarkable that, in this experiment, the red lead that is nearest to the glass becomes white first; and it adheres to the glass so closely, as to require the edge of a sharp knife to scrape it off.

I thought that by filling the set of phials with red lead, and making the vapour pass through them all in succession, I should, in the easiest manner, get a quantity of this new kind of white lead. But I only found, after continuing the process a considerable time, that the red lead in the first phial became very slightly white, just at the bottom where the vapour entered it, and also in a circle close to the glass at the top; and that near the top of the second phial there was a similar circle, but not near so white, while the rest of the lead was of a darker

colour.

colour. But the whole quantity was considerably increased in weight by this means.

Being willing to examine what *air* this white lead would yield, I first put a quantity of it into a gun barrel, and presently found that it yielded a very great quantity of air, the first produce of which was nitrous, and the last dephlogisticated. After the process the materials had become *lead*, exactly as red lead used to do in the same degree of heat.

I then put into a glass vessel a quantity of this white lead, weighing one ounce, one pennyweight, seventeen grains. The produce was forty four ounce measures of air; of which a small part at the first was phlogisticated, but all the rest was exceedingly pure. After the process the materials weighed seventeen pennyweights, as near as I could estimate it, for the lower part of the materials were vitrified, and could not be separated from the bottom of the glass vessel. The rest of the materials that had not been vitrified were of the same consistence, and colour, with that which remains after the same process with red lead, with or without spirit of nitre, viz. the upper part red, and the lower yellow.

Though this white lead was perfectly dry and brittle, a considerable quantity of moisture came over during the process, at first transparent, but afterwards yellow; and the inside of the glass tube through which the air and moisture were conveyed, was

coated

coated with a white substance, but next to the glass vessel it was yellow. The air had been exceedingly turbid, and the water employed to collect it was very acid, and yielded much air; which, no doubt, was nitrous.

Another course of experiments on the presence of nitrous acid in the calces of metals, throwing light on the impregnation of minium with phlogisticated nitrous acid vapour, I shall insert them in this place.

All the nitrous metallic salts have been distinguished by their property of *deliquescence*; but in my experiments with a long continued sand heat, I produced two of these saline substances, which did not deliquesce at all. They were produced from diluted solutions of copper and of mercury in the nitrous acid. The crystallizations were formed during the action of heat, in glass vessels hermetically sealed; and they were dissolved again in the same menstruum, when it was cold. But when the vessels were broken, and the saline substances were exposed to the air, they attracted no humidity at all; and yet they were not mere calces, because they were exceedingly caustic, and had a most disagreeable taste. I have since produced a saline substance of this kind from *iron* in a much less space of time, and the examination of it may throw some light on the constitution of the others.

A diluted

A diluted solution of iron in nitrous acid, being only exposed one day to a pretty strong sand heat, in a glass tube hermetically sealed, all the iron seemed to be precipitated, and the liquor was left nearly colourless. This liquor afterwards dissolved iron as before, so that the action of heat in these circumstances, viz. under a strong pressure, and when nothing can escape into the open air, seems to oblige the acid to quit its hold of the metal, in a great measure. It is indeed the property of nitrous solutions of iron, that they will always make a deposit, and then dissolve more iron, I believe without limit; but then the colour of the acid always continues red.

By this process, therefore, this remarkable property of the nitrous acid seems to be increased with respect to iron, and may perhaps be extended to the other metals.

The iron precipitate was by no means a mere calx; for it had a very acrid taste.

With copper a considerable time seems to be absolutely requisite to produce these non-deliquescent crystals, as appears from the following experiment which was likewise attended with some other circumstances, that I am not able to explain. A quantity of a weak, but saturated solution of copper in spirit of nitre, which had been exposed to a sand heat about a week, and in which some crystals were

were formed, had many more crystals formed in it; so as to become like a thin paste, presently after it was poured out of the tube. But when the whole mass was dissolved by heat, in the open air, and then dried, it became perfectly deliquescent; unlike that which had crystallized before in a longer continued heat.

That excellent philosopher, and most amiable man, Mr. Fabroni, who is as communicative as he is intelligent, informed me that the calx of tin would dephlogisticate spirit of nitre, and leave it colourless. This I found to be true; but then I found that, together with its colour, the acid lost almost all its strength. And trying other metals, I presently found that the earths of all of them have a remarkably strong affinity with the nitrous acid, and firmly uniting with it and a little water with which it is combined, make together a perfectly dry substance, quite unlike what it was before; the water being no more apparent, than it is in dry slaked lime. But heat will discover the water in both the cases.

Of this kind of calx, which I think we may properly term *nitrated*, is the white minium, which I had before procured by saturating red lead with nitrous vapour; the phenomena of which, as I have found them to extend to other metals, I now understand better than I did before. I thought it
some-

something extraordinary, that a red substance, like minium, should, by the addition of a red and highly phlogisticated vapour, become a white substance. But I find that all the metallic calces on which I have tried the experiment do also become white, when they are, in like manner, saturated with spirit of nitre; and that this may be effected by a much easier process than I thought of before.

The production of the red vapour of spirit of nitre by means of bismuth, and other metals of which it makes a rapid solution, will be a difficult and unpleasant process to most persons; and those who are most expert in experiments of this kind, will be obliged to make several trials before they succeed to their wish, in some of the experiments that I have reported. But I now make all these nitrated calces by means of the simple distillation of weak saturated solutions of any of the metals.

In this process the greatest part of the water is evaporated, and the acid, together with a small portion of the water, firmly unites with the calx of the metal, and, together with all the phlogiston that the metal contained, is deposited in the form of a white powder, which is incapable of being re-dissolved, either in the same menstruum, or in water. This deposit of white matter is made during the whole course of the distillation, in which nothing comes

comes over but water; and the whole of the metallic calx becomes a white nitrated powder, as described above. This, at least, is the case with copper; and though I did not make the experiment in the same manner with *tin*, the phenomena, in a similar process, were the very same. There will probably, however, be considerable differences when the process is extended to other metals.

In distilling a quantity of that solution of copper, which remains after making nitrous air (of which about one twentieth part is strong spirit of nitre, and the rest water) but fully saturated, there came over a transparent liquor, which had little or no taste; and from the very beginning of the process, I observed a constant deposition of white matter, which kept increasing, till the greatest part of the fluid was expelled. This matter I collected and dried, when it remained a perfectly white powder, but was easily discovered to contain much concentrated nitrous acid. For when I exposed it to heat in a glass tube, it emitted a copious red vapour, together with a good deal of liquid, and exhibited all the phenomena that I had before discovered in white nitrated minium, and in the calx of tin, on which I had distilled spirit of nitre. For, beginning with the idea that Mr. Fabroni had given me, I first put the spirit of nitre upon the calx of tin, and afterwards upon the tin itself; but I had the same

same produce of white nitrated powder at the last. That calx of tin which was yellow was made perfectly white by the distillation of spirit of nitre upon it.

The experiment of lead I made in a different manner, as follows. I dissolved seven pennyweights of lead, in spirit of nitre mixed with about an equal quantity of water, when some air was produced, but not much. The bulk of what remained was a white powdery substance, covered with a small quantity of liquid, at first green, but afterwards transparent. Transferring the whole into a cup, and rinsing the phial in which the solution had been made, I observed that the white substance, which was *nitre of lead*, was immediately dissolved by the water. Placing the cup in which the whole was contained near the fire, it became almost all liquid, and transparent, the menstruum being enabled by heat to hold in solution a much greater quantity of this nitre of lead.

When by this exposure to heat, all the moisture was evaporated, and it was made perfectly dry, it weighed eight pennyweights, so that there was an addition of one pennyweight from the acid and the water that were now latent in this calx. In this manner, however, it was brought to the same state with the nitrated calces of copper and tin above-mentioned. For when heat was applied to this
white

white substance, a red vapour was expelled from it, but seemingly combined with more water.

Having, in this, or some other similar manner, procured white nitrated calces of *lead*, *zinc*, *copper*, and *tin*, I inclosed a little of each in separate glass tubes, and then, with a blow pipe, applied to them the flame of a candle; when they all emitted red vapour, and as soon as the tubes were quite filled with it, I closed them all hermetically, before any air could be admitted.

Letting these tubes remain some days, I observed that the red vapour was re-absorbed by all the calces, but less slowly by the calx of lead than by those of tin or copper, and most quickly by that of zinc. N. B. I found it exceedingly difficult to expel all the moisture from the solution of zinc in spirit of nitre; but when this was effected, I had a true nitrated calx of this metal, as well as of the rest.

This experiment discovered to me a mistake I was under with respect to my last directions for filling of glass tubes with the red vapour of spirit of nitre. Instead of doing it directly, from the solution of bismuth, which is a difficult and disagreeable operation, I advised to procure, in the first place, a quantity of what I now call *nitrated calx of lead*; and putting some of it into a glass tube, closed at one end, to heat it till the whole tube be filled with

with the red vapour, and then immediately to seal it hermetically. This direction will still be right, provided that presently afterwards that end of the tube which contains the nitrated calx of lead be taken off, by melting the tube just beyond it, which indeed I then also advised to do, though I had not discovered the principal reason for it. For if the white calx from which the red vapour was expelled be suffered to remain long in the tube, it will re-imbibe the whole of it. But then the vapour may be expelled again by heat, and will continue to fill the tube a considerable time.

When I first produced the nitrated calx of lead, it was by means of a rapid solution of pieces of bismuth; and the vapour was conveyed immediately from the vessel in which the solution was made, through a bent tube connected with it, into the other vessel, in which I had placed the red lead. But this vapour, as I then observed, was by no means dry; and small drops of a very blue spirit of nitre were frequently falling from the end of the tube out of which the vapour issued. This degree of moisture I find greatly facilitates the absorption of the vapour.

Willing to try the effect of a perfectly *dry* nitrous vapour, I made the solution with the apparatus described Pl. V. fig. 3, interposing one of the inverted phials between the two vessels that I made

use of before; and at first I concluded that this dry vapour would not be imbibed by the minium at all. But I found, after some days, during which it had been confined in a phial with a ground stopper, together with some minium, that it was completely absorbed, and the red lead became white as before.

SECTION XVIII.

Of the firing of inflammable Air in the Vapour of nitrous Acid.

CONSIDERING inflammable air at first as air united to, or loaded with phlogiston, I exposed to it several substances, which are said to have a near affinity with phlogiston, as oil of vitriol, and spirit of nitre (the former for above a month) but without making any sensible alteration in it.

I observed, however, that inflammable air, mixed with the fumes of smoking spirit of nitre, goes off at one explosion, exactly like a mixture of half com-

mon and half inflammable air. This I tried several times, by throwing the inflammable air into a phial full of spirit of nitre, with its mouth immerfed in a bafon containing fome of the fame fpirit, and then applying the flame of a candle to the mouth of the phial, the moment that it was uncovered, after it had been taken out of the bafon.

This remarkable effect I haftily concluded to have arifen from the inflammable air having been in part deprived of its inflammability, by means of the ftronger affinity, which the fpirit of nitre had with phlogifton, and therefore I imagined that by letting them ftand longer in contact, and efpecially by agitating them ftrongly together, I fhould deprive the air of all its inflammability; but neither of thefe operations fucceeded: for ftill the air was only exploded at once, as before.

And laftly, when I paffed a quantity of inflammable air, which had been mixed with the fumes of fpirit of nitre, through a body of water, and received it in another vefiel, it appeared not to have undergone any change at all, for it went off in feveral fucceffive explofions, like the pureft inflammable air. The effect above-mentioned muft, therefore, have been owing to the fumes of the fpirit of nitre fupplying the place of common air for the purpofe of ignition, which is analogous to other experiments with nitre.

I have

I afterwards diversified this experiment, and observed several new circumstances relating to it, and some of the results are sufficiently curious. They fully confirmed my former observation, with these additions, that spirit of nitre takes phlogiston from inflammable air, becoming of a deeper colour by this communication with it; that when the spirit of nitre is weak, and phlogisticated, as when it is blue, or green, the inflammable air agitated in it resembles a mixture of nitrous and inflammable air; and that when the spirit of nitre is strong, and very pure, inflammable air agitated in it explodes in the same manner as it does in conjunction with dephlogisticated air. And, lastly, what is as remarkable as any of these facts, is that this effect of the nitrous acid is of no long continuance. The facts, as I observed them, were as follows.

Having filled a tall jar with strong yellow nitrous acid, inverted in a basin of the same, I displaced the whole of it, by introducing a quantity of inflammable air; and then applying a candle to the mouth of it, the flame descended from the top to the bottom. At the same time the jar was filled with a red vapour; and having repeated the experiment several times with the same acid, the whole of it became much redder than it was before. A jar being half filled with this acid, a candle let down

into it burned naturally, but I think rather better than in the open air.

In the colourless spirit of nitre, which is the least impregnated with the acid, the explosion was not different from what it would have been if the phial had been previously filled with water only.

When this experiment was made with the green or the blue spirit of nitre, and especially the latter, which is less acid than the former, the candle burned with a blue lambent flame in the neck of the phial, just as if a small quantity of nitrous air had been mixed with the inflammable.

In the spirit of nitre which is *green tinged with yellow*, which is the utmost effect of the impregnation of water with the nitrous acid, the inflammable air went off with a loud explosion, almost like a mixture of inflammable and dephlogisticated air.

In the preceding experiments, the inflammable air was fired immediately after it had displaced the spirit of nitre, without allowing any time for their mutual influence, and without agitation. In the following experiments the effects of those circumstances were tried.

Having introduced a quantity of inflammable air into a phial previously filled with strong spirit of nitre, inverted in a basin of the same, and allowing

lowing it to continue in that situation, I observed that the air soon began to increase in bulk, and in a few hours it was one sixth more than it had been. The next morning, when I observed that it had increased very little more, I agitated it; when, in about a minute, it was increased again one fifth, but more agitation hardly produced any sensible effect. Upon this I applied to it the flame of a candle, when it went off with a very loud explosion, in all respects like a mixture of dephlogisticated and inflammable air. Going over the same process with blue spirit of nitre, the increase was much quicker, and more considerable; in all, one fourth of the whole. But it was reduced by agitation precisely to its former dimensions, and when I applied the flame of a candle to it, it burned with a green flame, exactly like a mixture of nitrous and inflammable air.

It is easily inferred from these experiments, that the strong yellow spirit of nitre, which contains the most acid with the least phlogiston, supplies the inflammable air with a species of *vapour*, that, by readily uniting with its phlogiston, promotes the accension of it, and thereby increases the force of its explosion; whereas the weaker and phlogisticated acids seem to impart to it an additional quantity of phlogiston, making it to be, in part, nitrous air.

It is very remarkable that, whatever be the effect of the nitrous acid on inflammable air in the circumstance above-mentioned, it is not in the smallest degree permanent; and that nothing belonging to the acid enters properly into the constitution of the inflammable air, so as to remain with it. For if it be transferred from the phial out of which it had expelled the nitrous acid, and in which it would have exploded all at once, into another phial, passing through a body of water, it immediately becomes the same thing that it had been before, making a great number of small explosions in the mouth of the phial only, as I had observed formerly, and also upon this occasion.

It is still more remarkable, that if the inflammable air continue long in the vapour of spirit of nitre, and be fired in it, without ever being removed from it, it returns to its former state.

A phial, three fourths of which contained inflammable air, in spirit of nitre (the phial being inverted in a basin of the same) which had stood about a week, and which I agitated a little before I tried it, burned upon the spirit of nitre exactly like inflammable air in other circumstances, making a great number of explosions. I also fired the air contained in two other phials, which had stood the same time without agitation, and they were both exploded

exploded exactly like the preceding. The colour of the flame was green.

Left this difference should have arisen from the quality of the spirit of nitre, I immediately filled a phial with the very same acid, and displacing it with inflammable air, found that it was fired all at once, with a very bright flame, as before; and at the same time another quantity, which I had agitated a little, made a louder explosion.

Being desirous of trying, what *space of time* was necessary to produce this remarkable change in the explosion of inflammable air in the vapour of the spirit of nitre, I first let a quantity of it continue one night only in those circumstances, and the next morning it was fired with one bright explosion. Another quantity was suffered to remain confined by spirit of nitre three days; when it burned at first with a greenish flame in the mouth of the phial, but immediately after a bright flame descended suddenly to the bottom of it.

SECTION XIX.

Of the Mixture of vitriolic and nitrous Acids.

A Mixture of oil of vitriol and spirit of nitre in equal proportions dissolved iron, and the produce was nitrous air; but a less degree of spirit of nitre in the mixture produced air that was inflammable, and which burned with a green flame. It also tinged common air a little red, and diminished it, though not much.

It is something remarkable, that when metals are dissolved in a mixture of spirit of nitre, and any other acid, they act, in a manner, independently of each other; and that the nitrous acid, acting more suddenly than the others, produces its greatest effect at the first, as the quality of the air discovers; the first produce in these cases being always chiefly, and sometimes almost wholly, nitrous. Afterwards there is an equal mixture of both, and the last produce is inflammable air. This will also be seen to be the case when these acids are impregnated with the nitrous vapour, and the metals afterwards dissolved in them,

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Using equal parts of oil of vitriol and spirit of nitre, diluted with water, in the solution of *iron*, the first produce, and the greatest part of the bulk of the air, was strong nitrous air, and the last was inflammable. I observed also, that all the nitrous air came very rapidly, as is usual in the solution of iron in spirit of nitre, but that the moment the effect of the spirit of nitre was over, the air came equably and moderately; and from this time the whole produce was inflammable air.

I also put equal quantities of oil of vitriol and strong spirit of nitre, diluted by water, upon *zinc*, when the first produce was fired with an explosion, exactly (as will be seen in its proper place) like the air produced from zinc by water impregnated with vitriolic acid air, and then with nitrous vapour; a vivid flame descending rapidly from the top to the bottom of the phial. But in the next produce the flame descended gradually, and in the last a candle burned quite naturally. In this experiment the effects of the two acids were not so distinguishable as in the preceding; but then it is to be observed, that the solution of zinc in the nitrous acid yields dephlogisticated nitrous air.

Because a mixture of nitrous acid will discharge the black colour from phlogisticated vitriolic acid, Mr. Beaumé infers that the former has a stronger affinity with phlogiston than the latter. He also

observes of this mixture that it will readily inflame oil of turpentine, but that nothing farther is known concerning it.

I would observe, however, that the vitriolic acid does likewise discharge all colour from the nitrous acid, and therefore, reasoning as Mr. Beaumé does, we might draw a conclusion the reverse of his. I would therefore rather say, that the two acids in conjunction have a different action upon phlogiston than they have when separate.

If the marine acid be mixed with the vitriolic, the marine acid air is instantly expelled, and the water is, I suppose, seized by the acid of vitriol. But when the vitriolic and nitrous acids are mixed, no such effect takes place. They, therefore, seem to occupy the water jointly, without either of them dislodging the other, at least in the space of some weeks. What more time will effect I have not yet seen.

If the nitrous acid be poured gently upon the vitriolic, strongly concentrated, they will continue unmixed for some time ; but, without any agitation, they will incorporate gradually, a white cloudiness being always seen where they are contiguous. When they are shaken together a small degree of heat will be produced, and numberless bubbles will be formed, which, however, are presently absorbed. There is also at first a whitish vapour over the surface

face of the mixture; and after some time, though both the acids be ever so pure, and the vitriolic has been distilled again and again, there will be a deposit of a white substance, which I have not yet examined.

I have observed that the yellow colour of the common spirit of nitre is discharged by a mixture of the vitriolic acid. When I poured a weak green spirit of nitre upon concentrated oil of vitriol, it became yellow where they were contiguous; but the quantity of nitrous acid being much greater than that of the vitriolic, it was green above, without any visible vapour on its surface. The next morning the nitrous acid was colourless, contiguous to the vitriolic, and the rest yellow.

Afterwards I poured upon concentrated oil of vitriol an equal quantity of that nitrous acid, which had first acquired a deep orange colour by heat, and then had become green by keeping. The effect was, that from green it instantly became yellow throughout, and continued distinct from the vitriolic acid six days. In one day they did not seem to affect each other in the least, but afterwards a cloudiness was observed, where they were contiguous to each other, which increased till almost the whole had that appearance; and when they were shaken together it was transparent like water.

In order to try the full power of the vitriolic acid to discharge the colour of spirit of nitre, I dissolved in the strongest spirit of nitre a quantity of copper, which gave it a deep green colour. But on mixing it with vitriolic acid it instantly became perfectly colourless, and the copper was precipitated in the form of a white powder.

I poured very gently a quantity of aqua regia, made by impregnating marine acid with nitrous vapour, on vitriolic acid, and at first it effervesced very much, and the lower part was of a turbid white, while the upper part retained its orange colour. After some time the mixture was of a light orange throughout. I have not yet made any farther observations upon it.

To try how strongly the nitrous acid vapour was retained in this mixture of the two acids, I exposed a part of the mixture to the heat of a common fire, in a long green glass tube hermetically sealed, and found that though I kept it boiling, it continued colourless a considerable time. Afterwards a red vapour was expelled from the mixture, and at length the whole tube was filled with it. But when it was cold the vapour was all absorbed again, and the mixture, which was then of a pale orange colour, became afterwards quite colourless, as at first. In this case dephlogisticated air was expelled by the heat,

heat, in consequence of which the remainder was left phlogisticated, and then the two acids affected each other as before.

Having, as is mentioned above, observed some pretty remarkable phenomena that attend the mixture of the nitrous and vitriolic acid, particularly a turbid appearance, and a white deposit, though both the acids were perfectly transparent, and thinking that this might possibly arise from some extraneous earthy matter, in the oil of vitriol, I repeated the experiment with a quantity which had been first distilled, and then concentrated, and with a nitrous acid the purest and the palest that I could make. But this mixture was attended with the same phenomena as before, namely with heat, and a turbid white deposit.

I collected a quantity of this white deposit, and found that it was compleatly dissolved in spirit of salt, and gave it a yellow colour; so that it seems to be something contained in this acid, and probably essential to it. The experiment deserves to be repeated.

One of the most extraordinary circumstances that I have hitherto observed relating to this mixture, is the extreme volatility that it seems to give to the nitrous acid, so that, as far as I can yet perceive, the whole of it makes its escape from the mixture.

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This observation was at first quite casual. For having left the mixture, consisting of equal quantities of the strongest kinds of each of these acids, in a phial with a ground stopper, about four months, in which I had been absent from home, I found, at my return, the stopper driven out, and nothing in the phial besides the vitriolic acid, and, as far as I could judge, quite pure. For when I dissolved iron in it, nothing but inflammable air was yielded, even from the beginning of the process, and no mixture of nitrous air at all. Also the vitriolic acid was much weaker than it had been; so that it had been diluted afterwards by imbibing water from the atmosphere.

I had the same result from another mixture of equal quantities of the two acids, which had stood in the phial without a stopper from the 6th of June to the 23d of July; and the quantity was diminished only one fourth of the whole.

I also exposed, during the same time, to the open air, some of the crystals which I had observed to be formed by the impregnation of the vitriolic acid with the nitrous acid vapour. The consequence was, that the crystals gradually dissolved, and the quantity of liquid increased, till it exceeded twice the bulk of the crystals. When I dissolved iron in this liquid, I got nothing but inflammable air.

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When the very first produce of it was mixed with common air, there was no sensible diminution of it, so that there seemed to be no nitrous air produced.

Another method of separating the nitrous from the vitriolic acid, and in much less time than the above, was by exposing the mixture to nitrous air. This I have observed phlogisticates nitrous acid, and renders it extremely volatile; so that a very great proportion of it escapes. And when it is mixed with the vitriolic acid, and exposed in the same manner, the whole of it seems to escape.

Having introduced a phial of this mixture into a jar of nitrous air, in the same manner as I had before treated the nitrous acid itself, I observed that it absorbed the nitrous air as fast as the pure nitrous acid alone had done. Immediately after the process was commenced, it was covered with a dense red vapour, and gradually assumed a light orange colour throughout, beginning at the top. When the whole of it had acquired this colour, I withdrew it, and exposed it twenty four hours to the open air; after which the top was become of a light blue, and the bottom of a yellowish colour. I then put it into another jar of nitrous air, and suffered it to remain there a fortnight, during which time I was absent on a journey.

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At my return I found the mixture quite colourless, though it had absorbed little more of the nitrous air. I then dissolved iron in it, and it yielded nothing but inflammable air, of the strongest kind, without the least mixture of nitrous air; the very first produce of it not in the least affecting common air. The water in the jar in which this process was made yielding air copiously, I collected a quantity of it, and found it to be strong nitrous air. It had been produced by the impregnation of the water with nitrous vapour.

In order to discover in what *time* this effect might be produced, I repeated the experiment, and found that after being exposed four days to nitrous air, it became colourless, and the air produced by it from iron was all inflammable air.

If the vitriolic and the marine acids be mixed, much, if not all, of the marine acid is presently expelled, in the form of marine acid air. I was willing to try what would be the effect of adding this acid to the mixture of the two others above-mentioned; and I observed that, when I had poured a small quantity of a perfectly colourless marine acid, very gently upon the other, presently after they had been mixed, and while they were yet turbid, the marine acid remained transparent upon them both; but the place of contact presently became of a beautiful

tiful yellow or orange colour, very small bubbles of air rising now and then from it.

The next morning the whole mixture was of a beautiful orange colour. When it was agitated, it frothed very much, and the air or vapour, escaped very rapidly, making, as it were, small explosions; but after every agitation the mixture seemed to be more viscid, the air escaping with more difficulty. After the agitation, it remained of a paler colour than before. Probably the marine acid air had been, in some measure, thrown out; and the next day it was perfectly colourless, like water.

Bits of paper and bits of wood were not sensibly affected by the mixture of nitrous and vitriolic acids; and they did not give it any colour; but a fly gave another quantity of it a brownish tinge, though not very soon. The next day, that in which the vegetable matter had been immersed was of a light blue; and that into which the fly had been put was still of an orange colour, and rather deeper than before. Three weeks after this, both these mixtures having been a long time quite colourless, I dissolved iron in them, and they both yielded inflammable air only; so that, if this be any proof of the absence of the nitrous acid, this acid had now entirely left the mixture.

A mixture of an equal quantity of nitrous and marine acid with iron, produced air which at first was the same as a mixture of nitrous and inflammable air, burning with a lambent green flame; but the produce of air kept continually approaching to the state of pure inflammable air, and was at length wholly so; except that, at the last, it exploded with a blue flame; and the same phenomena occurred when more air was produced, pouring more spirit of salt into the same phial containing the iron.

An equal quantity of spirit of nitre and *radical vinegar*, diluted with water, produced, from iron, air which burned with a blue flame; but using more vinegar, the colour of the flame disappeared, and the last produce was not at all different from common inflammable air.

SECTION XX.

*Experiments on the Transmission of the Vapour of Acids
through a hot Earthen Tube.*

I^N my experiments on the phlogistication of spirit of nitre by heat, it appeared that when pure air was expelled from what is called dephlogisticated spirit of nitre, the remainder was left phlogisticated. This I find abundantly confirmed by repeating the experiments in a different manner, and on a larger scale; and I have applied the same process to other acids, and liquors of a different kind. From these it will appear that oil of vitriol and spirit of nitre, in their most dephlogisticated state, consist of a proper saturation of the acids with phlogiston; so that what we have called the *phlogistication* of them, ought rather to have been called their *super-phlogistication*.

I began with treating a quantity of oil of vitriol as I had done the spirit of nitre, viz. exposing it to heat in a glass tube hermetically sealed, and nearly exhausted; and the result was similar to that of the experiment with the nitrous acid, with respect to

the expulsion of air from it, though the phlogistication not appearing by any change of *colour*, I did not in this method ascertain that circumstance. The particulars were as follows.

After the acid had been made to boil some time, a dense white vapour appeared in quick motion at a distance above the acid ; and though, on withdrawing the fire, that vapour disappeared, it instantly re-appeared on renewing the heat. When the tube was cool I opened it under water, and a quantity of air rushed out, though the acid had been made to boil violently while it was closing, so that there could not have been much air in the tube. This air, which must, therefore, have been generated in the tube, was a little worse than common air, being of the standard of 1.12, when the latter was 1.04. I repeated the experiment several times, and always with the same result.

That this air should be worse than common air I cannot well explain. But in my former experiments it appeared that vitriolic acid air injures common air ; and that in proportion as pure air is expelled from this acid, the remainder becomes phlogisticated, or charged with vitriolic acid air, clearly appeared in the following experiment.

Making a quantity of oil of vitriol boil in a glass retort, and making the vapour pass through a red hot earthen tube, glazed inside and out, and filled with

with pieces of broken tubes, I collected the liquor that distilled over, and found it to be the same thing with water impregnated with vitriolic acid air. The smell of it was exceedingly pungent, and it was evident that more of this air had escaped than could be retained by that quantity of water. The oil of vitriol used in this process was one ounce, nine pennyweights, eighteen grains, and the liquor collected was six pennyweights, twelve grains. When I collected the air that was produced in this manner, which I did not do at this time, it appeared to be very pure, about the standard of 0.3, with two equal measures of nitrous air.

At another time, expending one ounce, eleven pennyweights, eighteen grains of oil of vitriol, of the specific gravity of 1.856 (that of water being 1000) I collected nineteen pennyweights, six grains of the volatile acid, of the specific gravity of 1.340, and procured 130 ounce measures of dephlogisticated air of the purest kind, viz. of the standard of 0.15.

It is easy in this manner to collect a great quantity of dephlogisticated air; but the principal objection to the process is, that after using a few times, the earthen tubes become tender, and too easily break, especially in heating or cooling. It is also difficult to lute the retort containing the acid and the earthen

tube. The air produced in this manner is filled with the densest white cloud imaginable.

Going through the same process with spirit of nitre, the result was in all respects similar, but much more striking; the production of both dephlogisticated air and phlogisticated acid vapour being prodigiously quicker, and more abundant. Expending five ounces, eight pennyweights, six grains of spirit of nitre, I collected six hundred ounce measures of the purest dephlogisticated air, being of the standard of 0.2. I also collected one ounce, seven pennyweights, fourteen grains of a greenish acid of nitre, which emitted copious red fumes. All the apparatus beyond the hot tube was filled with the densest red vapour, and the water of the trough in which the air was received, was so much impregnated with it, that the smell was very strong, and it spontaneously yielded nitrous air several days, just as water does when impregnated with nitrous vapour. Perceiving the emission of air from this water after it had stood some time, I filled a jar containing thirty ounce measures with it, and without any heat it yielded two ounce measures of the strongest nitrous air.

Taking the specific gravity of the acid before and after this distillation, the former was to the latter, as 1471 to 1182. When the weight of the air produced

duced in this experiment, and that of the liquor distilled, is compared with that of the acid before distillation, it will appear that there must have been a great loss of acid vapour, which was either retained in the water of the trough, or escaped through it.

I do not see that these experiments can be explained but on the supposition that the most dephlogisticated oil of vitriol and spirit of nitre are, in a proper sense, saturated with phlogiston; and that when part of the acidifying principle is expelled in the form of air, the remainder is supersaturated with it.

To try whether the acid thus saturated with phlogiston was convertible into pure air by this process, I heated the liquor collected after the distillation of the oil of vitriol, that is, water impregnated with vitriolic acid air, and made the vapour pass through the hot tube; but no air came from it, and when collected a second time, it was not at all different from what it had been before, the specific gravity was also the same.

It is evident, however, though this process does not shew it, that the volatile vitriolic acid contains the proper element of dephlogisticated air; since by melting iron in vitriolic acid air, a quantity of fixed air, which is composed of inflammable and dephlogisticated air, is produced. Melting iron in nine ounce measures of vitriolic acid air, it was reduced

to 0.3 ounce measures, and of this 0.17 ounce measures was fixed air. I repeated the experiment with the same result, and putting the residuums together, found the air to be inflammable.

But the result was something different when I sent through the hot tube the liquor that I had collected in the process with spirit of nitre. No air, however, was produced at the first, nothing appearing besides a *red vapour*, that was wholly absorbed by water, or escaped through it into the atmosphere. But towards the end of the process, I collected ten ounce measures of dephlogisticated air. The quantity of the liquor expended was about two ounce measures. It may, however, be presumed that this small quantity of air came from some of the acid which escaped the action of the fire in the former process. Indeed its coming at the last only may be considered as a proof of this; as all the more volatile acid, which came over first, yielded no air.

I submitted a quantity of spirit of salt to both these processes, viz. exposing to a boiling heat in glass tubes hermetically sealed, and making the vapour pass through a red hot tube; but no air was produced in either case. In the former case the water rushed into, and completely filled, the tube when it was opened under water; and in the other process the liquor distilled was precisely of the same specific gravity, and no doubt in all other respects the same,

same, as before distillation; but the acid that remained in the retort was of less specific gravity, in consequence of the acid vapour being expelled by the heat in the form of marine acid air, which appeared not to be affected by a red heat.

Though in this process with spirit of salt, the result is different from that of those with oil of vitriol and spirit of nitre, yet there is an analogy among all these three acids in this respect, that the marine acid, like the volatile acids of vitriol and nitre, is made by impregnating water with the acid vapour; so that in its usual state it may be said to be phlogisticated as well as they.

It was evident that the water in the worm tub was much more heated by the distillation of the spirit of salt than by that of the oil of vitriol, and especially that of the spirit of nitre; so that much of the heat by which it had been raised in vapour must, in the latter case, have been *latent* in the *air* that was formed; whereas, in the other case, it was communicated to the water in the worm tub.

In one of the processes with boiling spirit of salt, in a glass tube hermetically sealed, I had the same white vapour dancing in the middle of the tube, as in the experiment with the oil of vitriol; but this tube burst, and I never had the same appearance again, though I repeated the experiment several times for the sake of it.

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The vapour of dephlogisticated marine acid, which Mr. Bertholet discovered, and with which water may be impregnated, as with fixed air being made to pass through the hot tube, became dephlogisticated air, as in the following experiment.

Having poured a quantity of spirit of salt upon some manganese in a glass retort, I heated it, as in the preceding experiments, with a proper apparatus both for receiving the distilled liquor and the air. I found seven tenths of the air was fixed air, and the remainder very pure dephlogisticated. The quantity I could not measure, on account of one of the junctures in the apparatus giving way; but I do not imagine that quite so much pure air could be got in this method as from the manganese itself in a direct process. The liquor received in this distillation resembled strong spirit of salt, in which manganese had been put.

This process immediately succeeding that in which the glass tube, joining the earthen tube, and the worm tub, was left full of black matter by the distillation of the alkaline liquor, the blackness presently vanished, and the tube became transparent as before. On this account, however, it is possible that I might receive less pure air than I should otherwise have done.

Distilled

Distilled vinegar submitted to this process yielded air, two thirds of which was fixed air, and the rest inflammable. Expending two ounces nineteen pennyweights of the acid, I got one ounce nineteen pennyweights of a liquor which had a more pungent smell than it had had before distillation. It had also some black matter in it, and some of the same remained at the bottom of the retort when the liquor was evaporated to dryness. The air I received was ninety ounce measures.

SECTION XXI.

Miscellaneous Experiments on nitrous Acid.

1. *Of the firing of Paper dipped in a Solution of Copper in nitrous Acid.*

HAVING been informed by Dr. Small and Mr. Bolton of Birmingham, that paper dipped in a solution of copper in spirit of nitre would take fire with a moderate heat (a fact which I afterwards found mentioned in the Philosophical Transactions) it occurred to me that this would be very
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convenient for experiments relating to *ignition* in different kinds of air; and indeed I found that it was easily fired, either by a burning lens, or the approach of red hot iron on the outside of the phial in which it was contained, and that any part of it being once fired, the whole was presently reduced to ashes; provided it was previously made thoroughly dry, which, however, it is not very easy to do.

With this preparation, I found that this paper burned freely in all kinds of air, but not in *vacuo*, which is also the case with gunpowder; and, as I have in effect observed before, all the kinds of air in which this paper was burned received an addition to their bulk, which consisted partly of nitrous air, from the nitrous precipitate, and partly of inflammable air, from the paper. As the circumstances attending the ignition of this paper in inflammable air were a little remarkable, I shall just recite them.

Firing this paper in *inflammable* air, which it did without any ignition of the inflammable air itself, the quantity increased regularly, till the phial in which the process was made was nearly full; but then it began to decrease, till one third of the whole quantity disappeared.

2. *Of the firing of Gunpowder in different Kinds of Air.*

Gunpowder is also fired in all kinds of air, and, in the quantity in which I tried it, did not make any sensible change in them, except that the common air in which it was fired would not afterwards admit a candle to burn in it. In order to try this experiment I half exhausted a receiver, and then with a burning-glass fired the gunpowder which had been previously put into it. By this means I could fire a greater quantity of gunpowder in a small quantity of air, and avoid the hazard of blowing up, and breaking my receiver.

I own that I was rather afraid of firing gunpowder in inflammable air, but there was no reason for my fear; for it exploded quite freely in this air, leaving it, in all respects, just as it was before.

In order to make this experiment, and indeed almost all the experiments of firing gunpowder in different kinds of air, I placed the powder upon a convenient stand within my receiver, and having carefully exhausted it by a pump of Mr. Smeaton's construction, I filled the receiver with any kind of air by the apparatus described Pl. II. fig. 14, taking the greatest care that the tubes, &c. which conveyed

veyed the air should contain little or no common air. In the experiment with inflammable air a considerable mixture of common air would have been exceedingly hazardous: for, by that assistance, the inflammable air might have exploded in such a manner, as to have been dangerous to the operator.

Sometimes, I filled a glass vessel with quicksilver, and introduced the air to it, when it was inverted in a basin of quicksilver. By this means I entirely avoided any mixture of common air; but then it was not easy to convey the gunpowder into it, in the exact quantity that was requisite for my purpose. This, however, was the only method by which I could contrive to fire gunpowder in acid or alkaline air, in which it exploded just as it did in nitrous or fixed air.

I burned a considerable quantity of gunpowder in an exhausted receiver (for it is well known that it will not explode in it) but the air I got from it was very inconsiderable, and in these circumstances was necessarily mixed with common air. A candle would not burn in it.

3. *Of a casual Production similar to Gunpowder.*

I took half an ounce of *lead-ore*, and having saturated it with spirit of nitre, I dried it,
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put it into a gun barrel, filled up to the mouth with pounded flint, and placed vessels filled with water to receive the air. The consequence was, that as soon as this mixture began to be warm, air was generated very fast, infomuch that, being rather alarmed, I stood on one side; when presently there was a violent and loud explosion, by which all the contents of the gun barrel were driven out with great force, dashing to pieces the vessels that were placed to receive the air, and dispersing the fragments all over the room; so that all the air which I had collected, and which was about a pint, was lost. The mixture, before it was put into the gun barrel, was betwixt white and yellow, and had very much the smell of sulphur; so that it was in fact a composition similar to gunpowder.

Being desirous to know what kind of air I had got by this process, I put the same materials into a glass phial, and putting it into a crucible with sand, disposed the apparatus for receiving the air in such a manner, that the explosion could not affect it. It did explode as before, but the air was preserved, and appeared to be very strong nitrous air, almost as much so as that which is procured by the solution of metals.

P A R T II.

EXPERIMENTS RELATING TO THE MARINE ACID.

SECTION I.

Of the Colour of the Marine Acid.

ALL the chemists, as far as I can find, who have written on the subject of the marine acid, speak of its *colour*, as of a thing essential to it, and never fail to mention this as a necessary part of its definition: "Thus Mr. Macquer, in his *Dictionary*, says, that this acid differs from the vitriolic in having *smell* and *colour*." He also says, it differs from the nitrous acid by its colour, "which is more yellow and less red."

In my early experiments I gave a good deal of attention to this subject, but at that time I had not been able to ascertain on what it is that the colour

of this acid depends. Sometimes, I observed, I had procured it quite colourless, especially when I made it by impregnating water with marine acid air, but at other times I was not able, though I endeavoured to do it, to procure it without colour. I have since, however, perfectly satisfied myself with respect to the colour of this acid, and can at any time make it as colourless as water itself, the colour always coming from some impregnation, generally, if not always, of some *earthy matter*; with almost every thing of which kind it unites, and from which it generally takes some colour or other. I can also instantly discharge any colour that this acid has acquired, and restore it again at pleasure, as will appear in the course of these observations.

As I always make my own spirit of salt, as well as my spirit of nitre, and was satisfied from my former observations, that colour is not essential to this acid, any more than to the nitrous, or the vitriolic; on the first of August, 1777, having occasion for a quantity of spirit of salt, I was determined to make the distillation with all the attention that I could give to it, taking the produce at different times, which is my general custom, and which has been the occasion of my making a variety of important observations. I also received the superfluous vapour, or marine acid air, with the same

precautions, and in the same manner. The apparatus was nearly the same with that of which a drawing is given in Pl. V. fig. 4. The retort only being much larger, and using phials with water instead of the cup g. In this process also I seldom make use of any adopter.

Every thing being thus prepared, and having luted the vessels with a mixture of clay and fine sand, I began the distillation; and observed that the first produce was straw coloured, as usual; but all that came afterwards was quite colourless, like water. Also, all the impregnations of the water with the superfluous vapour were colourless. But the heat happening to abate towards the end of the process, a quantity of water rushed suddenly from the phial that received the impregnation, through the receiver, into the phial that contained the distilled acid; when all the acid that was in it, which was then quite colourless, immediately assumed as deep a straw colour, as that of the first produce of the distillation.

This process might have been sufficient to explain to me the whole mystery of the colouring of the spirit of salt; but it did not, and all the real advantage I gained by it was having in my possession a large quantity of pure colourless spirit of salt, to which I might endeavour to give colour in future

ture experiments. For all the hypothesis that occurred to me from considering the phenomena of this process was, that the colouring of this acid, as in most other cases, and especially in spirit of nitre, was owing to *heat*, or *phlogiston*; so that I was misled by the general maxims of the chemists, and also by the analogy of the two acids, and, indeed, that of the vitriolic acid also, which is known to acquire its black colour from substances containing phlogiston.

Thus I considered the colour of the first produce of spirit of salt, in the above-mentioned process, as similar to the usual colour of the first produce in the distillation of spirit of nitre, viz. to some unobserved phlogistic matter in the materials: and I considered the deep straw colour at the last, as occasioned, likewise, by some phlogistic matter driven into the vessel by the sudden rushing in of the water. Besides, I had more than once found spirit of nitre to become instantly of a deep green by a similar rushing of water into the recipient.

Conceiving that it must be phlogiston that gave colour to this acid; as well as to the nitrous acid and the vitriolic, I imagined I had nothing to do but to discover the proper mode of combining them; and I made trial of several things for that purpose, as putting into the colourless acid bits of charcoal, quenching hot charcoal in it, and mixing with it various other substances containing phlo-

giston, both hot and cold, but all without any effect.

As I had given colour to spirit of nitre by merely heating it in glass tubes hermetically sealed, I submitted the spirit of salt to the same trial; and for some time imagined that I had succeeded. For, in several instances, the spirit of salt did become coloured in these circumstances.

About half an ounce measure of colourless spirit of salt being confined in a glass tube an inch in diameter, and three feet long, hermetically sealed, on being exposed to heat, presently assumed the deepest usual colour of spirit of salt. Suspecting that there might have been some unperceived bit of straw, or some such thing in the large tube, I took a small one that was perfectly clean; and preparing it in the same manner, I exposed it to the heat of a common fire, and with the very same result. The acid had acquired a perfect straw colour.

But I was more confirmed in my opinion that it was heat, or phlogiston, or both, that produced this effect by finding that I got a peculiarly deep straw colour when I had inclosed the spirit of salt in a tube in which some oil had been before exposed to heat in the same manner, and to which a little of it adhered: and, what I had not much attended to before, I now observed that the acid retained

retained this straw colour when it was quite cold. But, notwithstanding these promising appearances, my hypothesis was totally overturned by finding, a day or two afterwards, that when I had exposed two glass tubes, in all respects, as nearly as I could judge, alike, containing the same colourless spirit of salt, to the same fire, and the same length of time, only one of them acquired the straw colour, while the other continued colourless, as at first. I examined both these tubes with the greatest attention, but could not discover any cause of this difference. There was indeed, more of the earthy matter, of which I shall treat presently, in the tube in which the acid was coloured, but that in which the acid continued colourless had a small crack in it, out of which some of the acid had oozed, so that I did not attribute this difference of colour to that circumstance.

At length, on the 6th of September, I discovered, by the merest accident, the whole mystery of what I had been so long, and so intently investigating. For, having some other use for the phial which contained the spirit of salt, I poured it into another phial, in which there had formerly been some iron filings and water, and the sides of which had a slight incrustation of ochre, which is known to give to glass a tinge that is not easily got out; but the moment that the colourless spirit of salt

touched this red incrustation, it became of a deep straw colour, and the phial wherever it had been touched by the acid, was perfectly clean.

After this it was impossible not to conclude that the colour of spirit of salt is not owing to phlogistic matter, like the colour of oil of vitriol, or that of spirit of nitre, but to an impregnation of some earthy matter, with which it is known readily to unite; and farther observations presently placed this hypothesis beyond all possible doubt. I was now also satisfied, that the first produce of spirit of salt, in the process above-mentioned, must have touched some of the clay, or sand, with which the vessels had been luted, and that the water, in its violently rushing into the receiver, must have met with more of it, though at that time, suspecting nothing of the true cause of the phenomenon, I did not perceive it.

SEC.

SECTION II.

Of the Impregnation of Marine Acid with various earthy Substances.

HAVING now observed the power of the marine acid to dissolve earths, I was desirous of examining the circumstances attending various solutions of this kind, both with respect to the earths themselves, and the colour of the saturated acid.

Spirit of salt dissolves a great quantity of *rust of iron* with effervescence, but not with much heat. The mixture was of a very deep brown, and what was not dissolved was of a dirty blackish colour. But possibly this might be owing to the rust of iron not being perfectly free from all foreign matters. The spirit of salt thus saturated with the rust of iron dissolved iron filings, and produced inflammable air; after which it was green. Having saturated a quantity of spirit of salt with the rust of iron, I evaporated it to dryness, when all the fluid part was dispersed in colourless fumes, and the ochre was left behind, and was re-dissolved by fresh spirit

of salt. I would observe, by the way, that spirit of salt is of excellent use to clean glass vessels tinged with the rust of iron, and many other matters. This may possibly have been known to others. To me the observation was casual, but of great value.

This acid dissolved a large quantity of *flowers of zinc* with great heat and effervescence. During the solution the acid became of a turbid black colour, but when it stood to subside, the black matter floating in it was deposited upon a mixture of black and white matter at the bottom of the phial, and the saturated acid was quite colourless, exactly like water. Also when I put flowers of zinc to spirit of salt deeply coloured with the rust of iron, the acid became colourless again.

Minium became white by the affusion of the spirit of salt, which acquired from it a beautiful yellow colour. A great quantity of it was dissolved, though more of it remained undissolved than of the flowers of zinc. When the red colour of the minium was quite discharged, fresh spirit of salt, though it dissolved, and became saturated with the white minium, acquired no colour from it.

When I had frequently washed a large quantity of minium in spirit of salt (though not till no more of it would have been dissolved) I put it into a green glass retort, and exposing it to as much heat

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as the glass would bear, I got from it hardly any fixed air, but about as much dephlogisticated air as I imagine it would have yielded before any spirit of salt had been applied to it. It seems, therefore, that the spirit of salt has no power of affecting its property of yielding dephlogisticated air. The matter melted into a red fluid substance, which, when cold, expanded, and broke the retort. This residuum gave a yellow tinge to spirit of salt.

Spirit of salt, I have observed, dissolves a great quantity of minium. In order to discover what became of the dephlogisticated air it contains, I distilled a quantity of that solution, which was of a yellow colour, made by the first affusion of the acid. When the solution became hot it yielded a quantity of dephlogisticated air, mixed with a very small quantity of fixed air, so as to make lime water turbid only in the slightest degree. As it boiled, no air at all was procured, nor when it was distilled to dryness.

I treated in the same manner a saturated solution of white minium, made so by its colour having been discharged by a previous affusion of the acid. But this solution yielded no air at all from the beginning to the end of the process. Nor was the common air in the retort phlogisticated either at the beginning or the end.

Spirit

Spirit of salt dissolved a great quantity of *red precipitate*, with great heat, but without effervescence. During the solution the acid was of a turbid white colour, and the precipitate is generally black, though some parts of it continued red till they were quite dissolved. But what remained undissolved at the last was all black. After it had subsided, all the opake matter was deposited, and the acid was beautifully transparent.

This acid dissolves a great quantity of *lapis calaminaris*, but not the whole of any part of it. The solution is made without heat, and it leaves no colour whatever in the spirit of salt.

Spirit of salt had no effect whatever on *crude antimony*, on *wolfram*, calcined or uncalcined, or on *white arsenic*. It is not affected by *vermilion* immediately; but in time it acquires from it a delicate yellow colour. It has also no sensible immediate effect on the black powder into which mercury is converted; but when lead is mixed with it, it, in time, acquires a deep orange colour from it. This must be produced by its separating the calx of lead from the superphlogisticated mercury, with which it is mixed.

All the above mentioned solutions are those of metallic earths, or other metallic matters, in spirit of salt. The following observations relate to the solution of *earthy substances* of a different kind in the same acid.

Colour-

Colourless spirit of salt dissolves completely a great quantity of very white *lime*, and is then of a straw colour; and the same was the effect of the solution of a pure lime from oyster shells. It also dissolved as much lime of a common sort, and was then of a true orange colour. But this seemed to be owing to a brownish matter in the lime, which was probably some earth of iron that was contained in it.

This acid dissolves a large quantity of calcined *magnesia*, and is then of a straw colour.

It does not sensibly affect *glass*, but when it was confined in a glass tube hermetically sealed, with a quantity of *pounded glass*, and exposed to a boiling heat, the glass seemed to be a good deal dissolved, and the acid became of a straw colour.

From *pipe clay* spirit of salt acquires a delicate yellow colour.

Wood *ashes*, out of which air had been expelled by heat, were dissolved in spirit of salt, and became black, but the colour of the acid was not changed.

The following substances were not sensibly affected by spirit of salt, *viz.* plaister of Paris, *stearites*, flint, *zeolyte*, fluor crust, *Moscovy talc*, cream of tartar, sedative salt, or borax. It had also no effect on the black matter that remains in the retort after the process for making ether.

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The black flakes, which remain after the solution of silver in spirit of nitre, are dissolved by spirit of salt, and impart to it a yellow colour.

It appears to me that it might be of considerable importance to the advancement of chemical knowledge to go through with the examination of all earthy substances in this manner, ascertaining whether they be soluble or insoluble in spirit of salt, and noting all the phenomena respecting either the earths themselves, or the acid, and comparing the results with the effects of other acids, &c. on the same earths. If any thing of this kind be done, at least to much extent, it is unknown to me.

SEC.

SECTION III.

Experiments relating to the Discharge of the Colour of various Solutions made by the Marine Acid.

I HAVE mentioned one instance in which a coloured spirit of salt had its colour discharged by a second saturation. Afterwards I accidentally found another substance that produced the same effect; and having had the curiosity to carry my observations relating to this subject to some length, I was fortunate enough to succeed in the investigation beyond what I expected, though much still remains to be ascertained with respect to it.

I had been extracting air from *cream of tartar* by means of oil of vitriol, first in a phial with a ground stopper, with very little heat, and then with a red hot sand heat. The black residuum I dissolved in spirit of salt, which was of the usual straw colour, and I found that, instead of giving any colour to it (which considering the blackness of the substance, I fully expected) it made it perfectly colourless like water; and, during the solution, I perceived a strong smell of liver of sulphur. Afterwards I had the same result from the residuum of a mixture of

oil of vitriol and cream of tartar, which had not been calcined. This matter being exposed to the open air attracted the moisture of the atmosphere very strongly, and had the consistence and smell of treacle. In time the more solid part formed itself into a cake, and pouring off the watery part, I dried the rest for other purposes.

After this I had the same effect from the mere *coal of cream of tartar*, calcined to blackness. The smell of this tartar, during the calcination, exactly resembled that of sugar or treacle. To spirit of salt, this coal, which was dissolved by it very rapidly, gave no colour whatever; but, on the contrary, discharged whatever colour it had acquired by any other impregnation; provided that, as in all the former cases, the colour was not too deep in proportion to the quantity of the coal of tartar. For the purpose of these experiments I happened principally to make use of a quantity of spirit of salt which had acquired a beautiful yellow colour from the solution of the white matter that remains after distilling to dryness a quantity of common oil of vitriol, the colour of this solution being easily discharged by a small quantity of the coal of tartar, and thereby answering my purpose remarkably well in the subsequent experiments.

Tartar calcined to whiteness (the black colour being expelled by long continued heat) had the same effect

effect on the coloured spirit of salt with the black coal of tartar, and was dissolved with equal rapidity. The power of this coal of tartar to discharge the colour, of spirit of salt was exhausted by being used for this purpose. For when it had discharged the colour of one impregnation, and was taken out, well washed, and dried, it had no effect a second time, It also lost this virtue by being washed with spirit of salt that had not been coloured with any impregnation.

The solution of salt of tartar in spirit of salt very much resembled the solution of the coal of tartar in it, and after the longest calcination that I ever gave the coal of tartar, it still yielded a great quantity of fixed air. But, notwithstanding this resemblance, the *salt of tartar* had no effect on the colour of this acid, neither was the colour sensibly affected by an impregnation with fixed air. It was not, therefore, the fixed air in the tartar that had produced this effect.

I have observed that the coal of tartar, during its solution in the spirit of salt, emitted a smell of liver of sulphur. This gave me the hint of trying liver of sulphur, and I presently found it answered my purposes much better than the coal of tartar itself, discharging instantly the deepest yellow colour that the acid ever acquired. It was evident, therefore, that the discharge of the colour was owing
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to something common to the coal of tartar and liver of sulphur, which I imagined to be phlogiston in some common state, an hypothesis which was rendered more probable by an experiment that will be recited presently; though it is certainly not favoured by the flowers of zinc producing the same effect.

The most remarkable circumstance relating to the discharge of the colour of spirit of salt is that, when it is exposed to the open air, it never fails to recover the colour that had been discharged, and a very little air confined in the same phial with it is sufficient for the purpose.

The first time that I observed this, was when I had coloured a quantity of spirit of salt with the residuum of oil of vitriol, which, as I have observed, gives it a yellow tinge, and had discharged the colour by the solution of black coal of tartar. For when I had, for some purpose or other, taken out the stopper of the phial in which it was kept, I found that, in a few days, it had completely recovered its former yellow colour.

When this process is made in a tall phial, it is pleasing to observe how the restoration of the colour begins at the top, and, in the course of a few days, descends gradually to the bottom. But let it be kept ever so long in a phial closed stopped, where no air can have access to it, and it will always continue

tinue colourless. I once kept a quantity of spirit of salt, first coloured, and then rendered transparent, in this manner, several months, in a phial with a glass stopper, and it continued colourless all the time; but upon taking out the stopper, it recovered in a few days its original colour, but more coal of tar-tar discharged this colour a second time.

I once had an instance of a quantity of this acid recovering its colour spontaneously in a manner that I cannot well account for. After the colour had been completely discharged, it had been confined in a phial with a glass stopper, and a very small quantity of air. In these circumstances it recovered its colour in two or three days; but, in a few days more, without having been opened in the mean time, it was found colourless again. I suppose there might remain enough of the black coal in the acid to discharge all the colour it had been able to recover by means of the air on its surface; but then why did not the same cause prevent its recovering its colour at all?

Something similar to this was the following observation. On the 19th of November 1778, having a quantity of spirit of salt which had acquired a deep yellow colour from various impregnations, I took two equal quantities of it, and putting them into equal phials, I discharged the colour of one of them with *liver of sulphur*, and that of the other

with *flowers of zinc*, observing that a large quantity of the latter was necessary for the purpose, but only a very small quantity of the former. In the discharge of the colour with the flowers of zinc I also perceived a slight smell of liver of sulphur.

These two phials, containing equally colourless spirit of salt, I covered with equal jars of common air standing in water; and in a day or two perceived that the acid in both of them had begun to recover its yellow colour; but that in which the colour had been discharged with flowers of zinc went no farther than about half way towards the bottom of the phial, and then the acid gradually became colourless again; whereas the acid in the other phial completely recovered its former colour. Thus they continued without any appearance of a farther change, till December 3, when I examined the air to which they had been exposed, and found it nearly in the same state in them both, and considerably worse than common air. With the air exposed to the phial with the flowers of zinc the measures of the test were 1.35, and with the liver of sulphur, 1.33. With the common air, at the same time, they were 1.2. Considering the difference of the circumstances in this experiment, I had expected a greater difference in the result.

Both liver of sulphur and flowers of zinc, I have observed, discharge the colour of spirit of salt. But when

when I discharged the colour of a quantity of this acid, made very yellow with various impregnations, with liver of sulphur, it recovered its colour by being exposed to the open air. On the contrary, though flowers of zinc produced the same effect, in discharging the colour of another portion of the same acid, the colour did not return by exposure to the air, not even though liver of sulphur was afterwards put to it.

It is evident from these experiments, that the colour of these impregnations arose from their imbibing dephlogisticated air from the atmosphere.

SECTION IV.

Of the Effect of a continued Heat on Spirit of Salt in Glass Tubes hermetically sealed.

HAVING made these solutions of earthy matters in spirit of salt, I exposed several of the saturated solutions, and other things into which the marine acid enters to a continued heat, and noted several remarkable effects of that process. But before I relate any of them, it will be proper to give

an account of the treating of pure spirit of salt in the same manner, besides what has been said of this process in a former section. In general, the spirit of salt, exposed to heat in glass tubes hermetically sealed, is enabled to do what it is incapable of in other circumstances, viz. to dissolve the glass itself, and more easily to seize upon metallic matters, as the calx of lead, and therewith to form a concrete substance, into which the acid itself enters.

On the 30th of August, 1777, I exposed to a boiling heat, in a glass tube about four feet long, and one third of an inch in diameter, as much spirit of salt as measured in the tube about an inch in length, and kept it boiling about two hours. After this the acid was still quite transparent, and the quantity not sensibly changed; but I observed that there was formed, as it cooled, a number of small crystals, perfectly white, at the bottom of the acid, and adhering to the sides of the tube. When I melted the end of the tube with a blow pipe, the pressure of the atmosphere forced the glass inwards. From this it was evident that there had been a decrease of elastic matter within the glass, which must have been produced by the incorporation of the acid vapour in the crystals that I have mentioned: for had it been a mere abrasion of the glass, besides that it would have been a powdery substance, and not in a concrete mass, the acid vapour would have been

set loose by the heat, and therefore would have pressed the softened glass outwards.

Making use of a tube an inch wide, and putting into it half an ounce measure of transparent spirit of salt, the crystals began to be formed in about an hour above the surface of the acid, and coated the tube about three inches, but all of it on the upper side, the tube having been placed in an inclined position.

When I exposed to the same heat the two tubes mentioned before, in one of which the acid was coloured, and the other not, I observed that more of this solid matter was formed in the former than in the latter, the acid having become coloured by dissolving the glass.

When any of these tubes happened to be cracked in the process, which was frequently the case, there was always a considerable incrustation formed on the outside of the glass, spreading from the crack, out of which the acid had escaped.

Having observed that, in proportion as this earthy, or rather saline matter was formed, the acid was diminished; to try whether there was any difference in the acid that remained from what it had been, I took it out of the tube in which it had been exposed to the heat, and exposed it again in a fresh tube; but I found that more saline matter was formed in this tube, exactly as in the former. I repeated the same process on the acid that

remained in the second tube, by putting it into a third, when more saline matter was produced; and this I repeated till very little liquid acid remained, though the tube broke, and a little remaining acid escaped, before I had quite finished my process upon it.

At length, however, I completely effected what I had been in pursuit of. For I exposed a quantity of acid in this manner till nothing liquid remained in the tube. This acid was distilled water impregnated with marine acid air, the quantity was about half an inch in length, in a glass tube a quarter of an inch in diameter. The lower part of the tube had a thick incrustation of white matter, and no more moisture remained within it, than what adhered to the sides of the tube, and would not run down it.

Though the acid continued to the last to dissolve the glass, it was evidently weakened by the continuance of this process, so that though both the marine acid air, and the water with which it was incorporated, had entered into the composition of the saline matter formed within the tube, there was in it more of the acid than of the water. Having extracted a considerable quantity of this saline matter from one of these tubes, I took out the remaining acid, and from a given measure of it, diluted with water, and bits of iron, I got three ounce measures of inflammable air; whereas from the same quantity of the same original spirit of salt I got, in the same circumstances 4.1 ounce measures. Allow-

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ance, however, must be made for the vapour that had escaped in pouring the acid into the tube, and out of it again.

In order to get a quantity of this saline matter, I kept a large tube with about an ounce measure of spirit of salt in the sand furnace near three months, and succeeded pretty well. It was all formed in or near the surface of the acid. The heat had been very moderate. For great care must be taken lest the glass should burst in this process. It seems, however, that when the heat is more considerable, the hotter acid may dissolve the concretioned saline matter that it comes into contact with, as appears in the following experiment.

Having exposed two pennyweights of colourless spirit of salt, in a long tube, about one third of an inch in diameter, the tube was presently incrustated about the length of nine inches with the saline matter, but very thin; and I observed that there was none of it within an inch of the surface of the fluid. Then making it boil more violently, I observed that whenever the hot acid reached the incrustation, it dissolved it, and washed the glass quite clean. By this means all the incrustation was presently washed off, and while the acid continued to boil, it did not appear again.

The reason why this incrustation was generally made at, or rather above the surface of the boiling

acid, seems to be, that the acid was there the most concentrated, on its expulsion from the water ; and this made a striking difference between these experiments, made with spirit of salt, and some which I made with water in the same manner. For when I bent the tubes at each end, and expelled the liquors by heat from one end of the tubes to the other alternately, I observed that with the spirit of salt the incrustations were always made above the surface of the boiling liquor ; whereas, in the tubes which contained water only, the incrustations were always made at the place from which the water last evaporated.

That the spirit of salt, in these experiments, dissolves the glass, and especially the lead that was in it, appeared from the following observation, which was first made by Mr. Magellan, who happened to be with me at the time. We had washed a quantity of this earthy, or saline matter, in distilled water ; when he observed that the water had the taste of *saccharum saturni*, and when the water that had been used in this manner was mixed with pump water, it turned it white, a manifest proof of its containing a solution of lead.

Spirit of salt not only dissolved this matter when it was hot, but also a considerable proportion of it when it was cold. When I had washed a quantity of it frequently with distilled water, till it was quite insipid,

insipid, it was not at all affected by oil of vitriol, or spirit of nitre; but when I had poured upon it some spirit of salt, and let them continue together a whole day, three grains of it were reduced to a grain and a half; so that half of it was dissolved by the spirit of salt, and the acid acquired a deep orange colour. As all the saline matter had been washed out of this substance by the water, what remained must have been the earth of the glass reduced to a powdery form, proper for the spirit of salt to act upon.

There was an incrustation of whitish matter when I made these experiments in the green or the black bottle glass, which has no lead in it, but it is manifestly of a different nature from that which is formed in the flint glass. The quantity is much less, and it differs from the other in several respects. When I dipped a large piece of a glass tube, completely covered with this incrustation, and which was perfectly white, in fresh spirit of salt, it presently disappeared, as if the acid had dissolved it all at once; and the incrustation seemed to imbibe the acid, as a wet sponge imbibes water: for when the lower part of it was dipped in the acid, it presently ascended, and moistened the upper part. But when I took this tube out of the acid, and dried it in the open air, the incrustation re-appeared, exactly as at first. - Also the acid in which it had been long plunged

plunged was not tinged by it, or only in the smallest degree imaginable.

This incrustation also adhered much more firmly to the green glass than to the flint, and when it was scraped off with the point of a knife, though it left the glass transparent, it was not quite so well polished as before: so that, probably, the glass had been, as it were, abraded, the texture being broken, but not so much as to make it separate from the tube.

I shall in this place mention an experiment similar to those above on the marine acid air itself. I buried a flint glass tube filled with this kind of air in hot sand, and let it continue there some weeks. When I took it out, it was covered with a white incrustation. I broke the end of the tube under quicksilver, and found that seven eighths of the whole quantity had been absorbed, and water imbibed about half the remainder. The very little that was left was phlogisticated air. This tube had been filled with so much care, that I cannot think there had been any common air in it.

I have several times repeated this experiment, and find that no great degree of heat is requisite to convert the marine acid air into this white substance. It is not at all affected by spirit of salt.

SECTION V.

Of the dephlogisticated Marine Acid.

SEVERAL years ago Mr. Woulfe informed me, that he thought that, by operating in my way, I should be likely to find something remarkable in the solution of *manganese* in spirit of salt; but, in a very friendly manner, he, at the same time, cautioned me with respect to the vapours that would issue from it, as, from his own experience, he apprehended it was of a very dangerous nature. He was also so obliging as to furnish me with a quantity of *manganese* for the purpose. I cannot say that it was the apprehension of danger, but rather having other things in view, that prevented my giving much attention to the subject at that time; and I should probably have deferred it still longer, had not Mr. Fabroni informed me of the dephlogisticating power of *manganese* with respect of spirit of salt, discovered by Mr. Bergman.

This information suggested a wish to procure a quantity of a perfectly dephlogisticated marine acid,

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in order to satisfy myself whether it would then yield any acid air, as it does in its common state, that is, when phlogistified; suspecting that it would not, as I had always imagined that a certain portion of phlogiston is necessary to all substances, and especially acids, assuming the form of air.

The experiments that I have made upon this subject give much weight to this opinion, and at the same time throw great light on the general doctrine of these kinds of air. For it appears that the marine acid, when it is deprived of its phlogiston, is brought into a state very nearly resembling the nitrous acid; being then incapable of being exhibited in the form of air, that is, of air capable of being confined by quicksilver. For the moment that the vapour, which then issues from it, is admitted to quicksilver, it unites with it, and forms a white powdery substance, in the same manner as the nitrous acid vapour does; and when I resume these experiments, I shall probably find that, with oily and other substances, this dephlogistified marine acid vapour will form compounds equally similar to those formed with them by the nitrous acid vapour. This is a new field that is yet before me.

From this analogy it is evident, that nothing is wanting to the nitrous acid vapour, to its assuming the form of *air*, but a sufficient quantity of phlogiston;

giston; and when it has got this phlogiston, it is *nitrous air*. This, therefore, is probably the nearest approach that we shall ever make towards bringing the nitrous acid into the form of air; and it is probably the combination of so much phlogiston with this acid, in the composition of nitrous air, that makes it not so readily absorbed by water, as the marine acid air, or vitriolic acid air; both which seem to be compounds exactly similar to that of nitrous air. I shall relate the experiments which led to these ideas in the order in which I made them.

I began with putting spirit of salt upon manganese, and then distilling it, as Mr. Fabroni had directed me; when the first observation that struck me, was a peculiar smell, exactly resembling that which is procured by dissolving *red lead* in the same acid. I then put a quantity of this distilled acid into a phial with a ground stopper, and a tube connected with it, and proceeded as I should have done to expel air from any other substance, with the flame of a candle, receiving the produce in quicksilver. On the application of heat, in these circumstances, it was easy to perceive that air, or vapour, was expelled; but it was instantly seized by the quicksilver, and formed a black crust.

Examining the air that was lodged at the top of the phial, and consequently had been mixed with
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this acid vapour, I found it very little, if at all, injured. This was owing to there being little or no phlogiston combined with the vapour, or separable from it.

I then fully impregnated a quantity of spirit of salt with manganese, by confining them together in the same phial; and I afterwards endeavoured to expel air from the acid thus altered. But still the vapour that came over immediately united with the quicksilver, and made a kind of amalgam with it, which, when dry, was a whitish or grey powder. The common air within the phial was not injured in this case, any more than in the former.

The above-mentioned powdery substance, being exposed to the heat of a candle on a piece of thin glass, evaporated in white fumes, but left behind it a small quantity of reddish matter, not very unlike red precipitate; which is another resemblance between the marine acid thus altered and spirit of nitre. After exposing this red matter for some time to a moderate heat, it became white, and sublimed without any sensible change. When it was exposed to the focus of a burning lens, upon quicksilver, it yielded no sensible quantity of air. I had imagined that, at least during the presence of heat, the acid, which was latent in this white substance, might have assumed the form of air, but I was disappointed in that expectation.

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The marine acid impregnated with manganese having the very same *smell* with this acid impregnated with red lead, I was led to repeat the preceding experiments with this substance also, and I had the same general results. For the vapour emitted by it instantly united with quicksilver, and formed with it a white powdery substance, of which, with a proper apparatus, I collected a considerable quantity.

Mr. Bertholet has shewn that what is called *dephlogisticated marine acid*, is that acid saturated with dephlogisticated air, which it gets from the manganese or minium. His discoveries relating to this subject are among the most brilliant that this age, so fruitful in discoveries, has produced,

PART

P A R T III.

OF THE PHOSPHORIC ACID.

HAVING made so many experiments on the acids, with a view to reducing them to the form of air, and upon their properties when exhibited in that new form, it might have been expected that I should, before this time, have taken notice of the *phosphoric acid*, which is so remarkably different from the other acids, and which bears so near a relation to the animal œconomy. The true reason of this seeming neglect of so important a subject of experiment was the expence necessary to procure it in any tolerable quantity. At length, however, I procured a quantity sufficient for a few experiments, not undeserving of being related.

Chemists do not need to be informed of the method of procuring this liquid acid from solid phosphorus; but for the sake of persons of only a general philosophical turn, like myself, it may be worth while to observe, that this acid is easily procured, with time, by exposing it to the open air in the mouth of a funnel, going into a phial which
receives

receives the acid, as the phosphorus gradually wastes by this kind of accension. It must be set in a place neither very cold, nor very warm. But this depends upon the consistence of the phosphorus, and other circumstances, which must be learned by experience. If it smokes very much, it is a sign that it is too warm, and is in danger of taking fire, in which case it may be saved by plunging it instantly in water.

Having procured my phosphorus, I first observed, that the water in which it had been long kept had nothing acid in it. For, being mixed with water made blue with the juice of turnsole, it did not affect its colour, which shews that no proper decomposition of it takes place in water. Having then exposed it to the open air, in the manner described above, I got a quantity of the acid with which I made the following observations.

With respect to *air*, this acid very much resembles radical vinegar, or rather the vitriolic acid. For though the application of heat converts it into vapour, it is all condensed again in the temperature of the atmosphere, and no part of it remains permanent elastic air. I made the experiment in a glass tube bent a little like a retort, the open end of which turned up into a vessel filled with quicksilver, and immersed in a basin of the same. When I made the acid boil, the vapour passed into the

recipient, but it was wholly condensed there, and the liquor so collected did not differ, as far as I could perceive, from what it had been before the evaporation.

As, like the vitriolic acid, this gave no air of itself, I thought that, like this acid, it might possibly give something similar to the vitriolic acid air by means of substances containing phlogiston. With this view I kept it in a boiling heat both with quicksilver, and also with spirit of wine, but without any effect; and even the common air, that was expelled from the phial in which the experiment was made, was not sensibly phlogistified.

This acid, however, resembled that of vitriol and radical vinegar in this, that it readily dissolved iron, especially with the aid of a little heat, and with it yielded a strong inflammable air. But there is something more remarkable in the produce of inflammable air from it by means of minium.

In order to try whether this acid had any of the properties of the nitrous, I mixed it with some minium out of which all the air had been expelled by heat. This substance, in this state, I had found, when mixed with nitrous acid, yields dephlogistified air, but no air at all with the vitriolic or the marine acid. The phosphoric acid mixed with this minium with little or no sensible heat, but the mixture

ture exposed to the flame of a candle yielded air very plentifully, and it was very turbid. I received it in lime water, but it did not precipitate the lime, except in the smallest degree. The air I got in this method was not affected by nitrous air, nor did it affect common air, but was strongly inflammable, burning with a bright white flame; and the smell of the air was the same with that of the strong smell of phosphorus. The massicot became of a darkish grey colour, or nearly black by this process.

Having a quantity of the mixture of phosphoric acid and spirit of wine, remaining from the experiment above-mentioned, and not being willing to lose it, I likewise mixed it with some of the same massicot, and I had the same result. The common air that was first expelled from the surface of the vessel in which the experiment was made was not much injured, the next that came had a small quantity of fixed air in it; but all the remainder was strongly inflammable, burning with a yellow flame, the next was more weakly inflammable, and the last produce was phlogisticated air only.

The phlogiston must have been supplied from the acid, since the massicot does not contain it.

About the time that I was making these experiments I was making observations on the exposure of a variety of fluid substances to a long continued heat. I therefore treated this acid in the same manner, first in a long glass tube, held in an inclined or nearly perpendicular position, and then in a horizontal one, expelling the acid by the heat from one part of the tube to the other; the result of which process was remarkably different from that of the other.

In a glass tube about thirty inches in length, and one third of an inch in diameter, I put as much of this acid as filled about an inch of the tube in length, and making it boil, there was a white vapour at the height of about fifteen or eighteen inches above the surface of the acid, continually dancing up and down as it boiled. At and below this part of the tube, it was very hot, but immediately above it was quite cold. I kept the acid boiling several hours without any sensible change.

Though the phosphoric acid was not changed by boiling several hours in the course of two days, in a glass tube hermetically sealed, and placed in nearly a vertical position, yet when I applied the flame of a candle to any part of the tube, after the acid had left it moist (when it had been made to flow to the other end) the glass was instantly covered
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ed with a white incrustation; and repeating this process, at each end of the tube alternately, I quickly made the whole solid. At least there was no more moisture in the tube than adhered to the sides of it, and could not be made to flow at all. This experiment I repeated in several tubes, and always with the same result, whatever was the quantity of the acid.

When the tube was made very hot there would sometimes be flashes of light in the inside, extending the whole length of the tube; and of these there were sometimes three in the same tube at different times. Whenever this happened, a part of the tube always acquired a thin coating of orange coloured matter, such as remains upon glass when phosphorus is really ignited upon it in the open air.

The white matter thus left in the glass tubes attracted no moisture from the atmosphere, at least no sensible quantity of it, and it was not at all affected by spirit of salt. It did not even long retain any sensible acidity; for when it had been washed several times, the water in which it lay did not even turn the juice of turnsole red.

If I be asked what becomes of the moisture which rendered the phosphoric acid liquid in this process, I should say that, as in the similar

experiments with the marine acid, it dissolves the glass, and with it the acid and water both unite in a solid form, as in other crystallizations; and since I made these experiments, I have been informed by Dr. Ingenhoufz, a man of a truly philosophical and experimental turn, that the phosphoric acid, when hot, dissolves glass, exactly like the fluor acid.

BOOK

BOOK IX.

EXPERIMENTS AND OBSERVATIONS RELATING TO VEGETATION AND RE- SPIRATION.

PART I.

OBSERVATIONS AND EXPERIMENTS RELATING TO VEGETATION.

SECTION I.

*Of the Restoration of Air in which a Candle has
burned out, by Vegetation.*

IT is well known that flame cannot subsist long without change of air, so that the common air is necessary to it, except in the case of substances, into the composition of which nitre enters; for these will burn *in vacuo*, in fixed air, and even un-

der water, as is evident in some rockets, which are made for this purpose. It is generally said, that an ordinary candle *consumes*, as it is called, about a gallon in a minute. Considering this amazing consumption of air, by fires of all kinds, volcanos, &c. it becomes a great object of philosophical inquiry, to ascertain what change is made in the constitution of the air by flame, and to discover what provision there is in nature for remedying the injury which the atmosphere receives by this means.

Having read, in the Memoirs of the Philosophical Society at Turin, vol. I. p. 41, that air in which candles had burned out was perfectly restored, so that other candles would burn in it again as well as ever, after having been exposed to a considerable degree of *cold*, and likewise after having been compressed in bladders (for the cold had been supposed to have produced this effect by nothing but *condensation*) I repeated those experiments, and did, indeed, find, that when I compressed the air in *bladders*, as the Count de Saluce, who made the observation, had done, the experiment succeeded: but having had sufficient reason to distrust bladders, I compressed the air in a glass vessel standing in water; and then I found, that this process is altogether ineffectual for the purpose. I kept the air compressed much more, and much longer, than
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the Count had done, but without producing any alteration in it. I also find, that a greater degree of cold than that which he applied, and of longer continuance, did by no means restore this kind of air: for when I had exposed the phials which contained it a whole night, in which the frost was very intense; and also when I kept it surrounded with a mixture of snow and salt, I found it, in all respects, the same as before.

It is also advanced, in the same Memoir, p. 41; that *heat* only, as the reverse of *cold*, renders air unfit for candles burning in it. But I repeated the experiment of the Count for that purpose, without finding any such effect from it. I also remember that, many years ago, I filled an exhausted receiver with air, which had passed through a glass tube made red hot, and found that a candle would burn in it perfectly well. Also, rarefaction by the air pump does not injure air in the least degree.

Though this experiment failed, I have been so happy, as by accident to have hit upon a method of restoring air, which has been injured by the burning of candles, and to have discovered at least one of the restoratives which nature employs for this purpose. It is *vegetation*. This restoration of vitiated air, I conjecture, is effected by plants imbibing the phlogistic matter with which it is overloaded

loaded by the burning of inflammable bodies. But whether there be any foundation for this conjecture or not, the fact is, I think, indisputable. I shall introduce the account of my experiments on this subject, by reciting some of the observations which I made on the growing of plants in confined air, which led to this discovery.

One might have imagined that, since common air is necessary to vegetable, as well as to animal life, both plants and animals had affected it in the same manner; and I own I had that expectation, when I first put a sprig of mint into a glass jar, standing inverted in a vessel of water; but when it had continued growing there for some months, I found that the air would neither extinguish a candle, nor was it at all inconvenient to a mouse, which I put into it.

The plant was not affected any otherwise than was the necessary consequence of its confined situation; for plants growing in several other kinds of air, were all affected in the very same manner. Every succession of leaves was more diminished in size than the preceding, till, at length, they came to be no bigger than the heads of pretty small pins. The root decayed, and the stalk also, beginning from the root; and yet the plant continued to grow upwards, drawing its nourishment through a black and rotten stem. In the third or fourth set of leaves,
long

long and white hairy filaments grew from the insertion of each leaf, and sometimes from the body of the stem, shooting out as far as the vessel in which it grew would permit, which, in my experiments, was about two inches. In this manner a sprig of mint lived, the old plant decaying, and new ones shooting up in its place, but less and less continually, all the summer season.

In repeating this experiment, care must be taken to draw away all the dead leaves from about the plant, lest they should putrefy, and affect the air. I have found that a fresh cabbage leaf, put under a glass vessel filled with common air, for the space of one night only, has so affected the air, that a candle would not burn in it the next morning, and yet the leaf had not acquired any smell of putrefaction.

Finding that candles would burn very well in air in which plants had grown a long time, and having had some reason to think, that there was something attending vegetation, which restored air that had been injured by respiration, I thought it was possible that the same process might also restore the air that had been injured by the burning of candles.

Accordingly, on the 17th of August 1771, I put a sprig of mint into a quantity of air, in which a wax candle had burned out, and found that, on the 27th of the same month, another candle burned perfectly

fectly well in it. This experiment I repeated, without the least variation in the event, not less than eight or ten times in the remainder of the summer.

Several times I divided the quantity of air in which the candle had burned out, into two parts, and putting the plant into one of them, left the other in the same exposure, contained, also, in a glass vessel immersed in water, but without any plant; and never failed to find, that a candle would burn in the former, but not in the latter.

I generally found that five or six days were sufficient to restore this air, when the plant was in its vigour; whereas I have kept this kind of air in glass vessels, immersed in water many months, without being able to perceive that the least alteration had been made in it. I have also tried a great variety of experiments upon it, as by condensing, rarefying, exposing to the light and heat, &c. and throwing into it the effluvia of many different substances, but without any effect.

Experiments made in the year 1772, abundantly confirmed my conclusion concerning the restoration of air, in which candles had burned out by plants growing in it. The first of these experiments was made in the month of May; and they were frequently repeated in that and the two following months, without a single failure.

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For this purpose I used the flames of different substances, though I generally used wax or tallow candles. On the 24th of June the experiment succeeded perfectly well with air in which spirit of wine had burned out, and on the 27th of the same month it succeeded equally well with air in which brimstone matches had burned out, an effect of which I had despaired the preceding year.

This restoration of air, I found, depended upon the *vegetating state* of the plant; for though I kept a great number of the fresh leaves of mint in a small quantity of air in which candles had burned out, and changed them frequently, for a long space of time, I could perceive no melioration in the state of the air.

This remarkable effect does not depend upon any thing peculiar to *mint*, which was the plant that I always made use of till July 1772; for on the 16th of that month, I found a quantity of this kind of air to be perfectly restored by sprigs of *balm*, which had grown in it from the 7th of the same month.

That this restoration of air was not owing to any *aromatic effluvia* of these two plants, not only appeared by the *essential oil of mint* having no sensible effect of this kind; but from the equally complete restoration of this vitiated air by the plant called *groundsel*, which is usually ranked among the weeds, and

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has an offensive smell. This was the result of an experiment made the 16th of July, when the plant had been growing in the burned air from the 8th of the same month. Besides, the plant which I have found to be the most effectual of any that I have tried for this purpose is *spinach*, which is of quick growth, but will seldom thrive long in water. One jar of burned air was perfectly restored by this plant in four days, and another in two days. This last was observed on the 22d of July.

In general, this effect may be presumed to have taken place in much less time than I have mentioned; because I never chose to make a trial of the air, till I was pretty sure, from preceding observations, that the event which I had expected must have taken place, if it would succeed at all; lest, returning back that part of the air on which I made the trial, and which would thereby necessarily receive a small mixture of common air, the experiment might not be judged to be quite fair; though I myself might be sufficiently satisfied with respect to the allowance that was to be made for that small imperfection.

SEC.

SECTION II.

Of the Restoration of Air infected with animal Respiration, or Putrefaction, by Vegetation.

THAT candles will burn only a certain time, in a given quantity of air is a fact not better known, than it is that animals can live only a certain time in it; but the cause of the death of the animal is not better known than that of the extinction of flame in the same circumstances; and when once any quantity of air has been rendered noxious by animals breathing in it as long as they could, I do not know that any methods have been discovered of rendering it fit for breathing again. It is evident, however, that there must be some provision in nature for this purpose, as well as for that of rendering the air fit for sustaining flame; for without it the whole mass of the atmosphere would, in time, become unfit for the purpose of animal life; and yet there is no reason to think that it is, at present, at all less fit for respiration than it has ever been. I flatter myself, however, that I have hit upon one of the methods employed by nature for this

this great purpose. How many others there may be, I cannot tell.

When animals die upon being put into air in which other animals have died, after breathing in it as long as they could, it is plain that the cause of their death is not the want of any *pabulum vite*, which has been supposed to be contained in the air, but on account of the air being impregnated with something stimulating to their lungs; for they almost always die in convulsions, and are sometimes affected so suddenly, that they are irrecoverable after a single inspiration, though they may be withdrawn immediately, and every method has been taken to bring them to life again. They are affected in the same manner, when they are killed in any other kind of noxious air that I have tried, viz. fixed air, inflammable air, air filled with the fumes of sulphur, infected with putrid matter, in which a mixture of iron filings and sulphur has stood, or in which charcoal has been burned, or metals calcined, or in nitrous air, &c.

As it is known that *convulsions* weaken, and exhaust the vital powers, much more than the most vigorous *voluntary* action of the muscles, perhaps these universal convulsions may exhaust the whole of what we may call the *vis vite* at once; at least that the lungs may be rendered absolutely incapable of action, till

till the animal be suffocated, or be irrecoverable for want of respiration.

If a mouse (which is an animal that I have commonly made use of for the purpose of these experiments) can stand the first shock of this stimulus, or has been habituated to it by degrees, it will live a considerable time in air in which other mice will die instantaneously. I have frequently found that when a number of mice have been confined in a given quantity of air, less than half the time that they have actually lived in it, a fresh mouse being introduced to them has been instantly thrown into convulsions, and died. It is evident therefore, that if the experiment of the Black Hole, at Calcutta, were to be repeated, a man would stand the better chance of surviving it, who should enter at the first, than at the last hour.

I have also observed, that young mice will always live much longer than old ones, or than those which are full grown, when they are confined in the same quantity of air. I have sometimes known a young mouse to live six hours in the same circumstances in which an old mouse has not lived one. On these accounts, experiments with mice, and, for the same reason, no doubt, with other animals also, have a considerable degree of uncertainty attending them; and therefore, it is necessary to repeat them frequently, before the result can be

absolutely depended upon. But every person of feeling will rejoice with me in the discovery of *nitrous air*, which supercedes many experiments with the respiration of animals; being a much more accurate test of the purity of air.

The discovery of the provision in nature for restoring air, which has been injured by the respiration of animals, having long appeared to me to be one of the most important problems in natural philosophy, I have tried a great variety of schemes in order to affect it. In these, my guide has generally been to consider the influences to which the atmosphere is, in fact, exposed; and, as some of my unsuccessful trials may be of use to those who are disposed to take pains in the farther investigation of this subject, I shall mention the principal of them.

The noxious effluvium with which air is loaded by animal respiration, is not absorbed by standing, without agitation, in fresh or salt water. I have kept it many months in fresh water, when, instead of being meliorated, it has seemed to become even more deadly, so as to require more time to restore it, by the methods which will be explained hereafter, than air which has been lately made noxious. I have even spent several hours in pouring this air from one glass vessel into another, in water, sometimes as cold, and sometimes as warm, as my hands

hands could bear it, and have sometimes also wiped the vessels many times, during the course of the experiment, in order to take off that part of the noxious matter, which might adhere to the glass vessels, and which evidently gave them an offensive smell; but all these methods were generally without any sensible effect. The *motion*, also, which the air received in these circumstances, it is very evident, was of no use for this purpose. I had not then thought of the simple, but most effectual method of agitating air in water, by putting it into a tall jar and shaking it with my hand.

This kind of air is not restored by being exposed to the *light*, or any other influence to which it is exposed, when confined in a thin phial, in the open air, for some months.

Among other experiments, I tried a great variety of different *effluvia*, which are continually exhaling into the air, especially of those substances which are known to resist putrefaction; but I could not by these means effect any melioration of the noxious quality of this kind of air.

Having read, in the Memoirs of the Imperial Society, of a plague not affecting a particular village, in which there was a large sulphur-work, I immediately fumigated a quantity of this kind of air; or (which will hereafter appear to be the very same thing)

thing) air tainted with putrefaction, with the fumes of burning sulphur, but without any effect.

I once imagined, that the *nitrous acid* in the air might be the general restorative which I was in quest of; and the conjecture was favoured, by finding that candles would burn in air extracted from saltpetre*. I therefore spent a good deal of time in attempting, by a burning glass, and other means, to impregnate this noxious air with some effluvium of saltpetre, and, with the same view introduced into it the fumes of the smoaking spirit of nitre; but both these methods were altogether ineffectual.

In order to try the effect of *heat*, I put a quantity of air, in which mice had died, into a bladder, tied to the end of the stem of a tobacco pipe, at the other end of which was another bladder, out of which the air was carefully pressed. I then put the middle part of the stem into a chafing-dish of hot coals, strongly urged with a pair of bellows; and, pressing the bladders alternately, I made the air pass several times through the heated part of the pipe. I have also made this kind of air very hot, standing in water before the fire. But neither of these methods were of any use.

* This was the first instance of my finding dephlogisticated air, but without knowing it to be at all different from common air.

Rarefaction and *condensation* by instruments were also tried, but in vain.

Thinking it possible that the *earth* might imbibe the noxious quality of the air, and thence supply the roots of plants with such putrescent matter as is known to be nutritive to them, I kept a quantity of air in which mice had died, in a phial, one half of which was filled with fine garden-mould; but, though it stood two months in these circumstances, it was not the better for it.

I once imagined that, since several kinds of air cannot be long separated from common air, by being confined in bladders, in bottles well corked, or even closed with ground stoppers, the affinity between this noxious air and the common air might be so great, that they would mix through a body of water interposed between them; the water continually receiving from the one, and giving to the other, especially as water receives some kind of impregnation from, I believe, every kind of air to which it is contiguous; but I have seen no reason to conclude, that a mixture of any kind of air with the common air can be produced in this manner.

I have kept air in which mice have died, air in which candles have burned out, and inflammable air, separated from the common air, by the slightest partition of water that I could well make, so that it might not evaporate in a day or two, if I should

happen not to attend to them; but I found no change in them after a month or six weeks. The inflammable air was still inflammable, mice died instantly in the air in which other mice had died before, and candles would not burn where they had burned out before.

Since air tainted with animal or vegetable putrefaction is the same thing with air rendered noxious by animal respiration, I shall now recite the observations which I have made upon this kind of air, before I treat of the method of restoring them.

That these two kinds of air are, in fact, the same thing, I conclude from their having several remarkable common properties, and from their differing in nothing that I have been able to observe. They equally extinguish flame, they are equally noxious to animals, they are equally, and in the same way, offensive to the smell, they equally precipitate lime in lime water, and they are restored by the same means.

Since air which has passed through the lungs is the same thing with air tainted with animal putrefaction, it is probable that one use of the lungs is to carry off a putrid effluvium, without which, perhaps, a living body might putrefy as soon as a dead one.

Insects of various kinds live perfectly well in air tainted with animal or vegetable putrefaction, when
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a single inspiration of it would have instantly killed any other animal. I have frequently tried the experiment with flies and butterflies. The *aphides* also will thrive as well upon plants growing in this kind of air, as in the open air. I have even been frequently obliged to take plants out of the putrid air in which they were growing, on purpose to brush away the swarms of these insects which infected them; and yet so effectually did some of them conceal themselves, and so fast did they multiply, in these circumstances, that I could seldom keep the plants quite clear of them.

When air has been freshly and strongly tainted with putrefaction, so as to smell through the water, sprigs of mint have presently died, upon being put into it, their leaves turning black; but if they do not die presently, they thrive in a most surprising manner. In no other circumstances have I ever seen vegetation so vigorous as in this kind of air, which is immediately fatal to animal life. Though these plants have been crowded in jars filled with this air, every leaf has been full of life; fresh shoots have branched out in various directions, and have grown much faster than other similar plants, growing in the same exposure in common air.

This observation led me to conclude, that plants, instead of affecting the air in the same manner with animal respiration, reverse the effects of breathing,

and tend to keep the atmosphere sweet and wholesome, when it is become noxious, in consequence of animals either living and breathing, or dying and putrefying in it.

In order to ascertain this, I took a quantity of air, made thoroughly noxious, by mice breathing and dying in it, and divided it into two parts; one of which I put into a phial immersed in water; and to the other (which was contained in a glass jar, standing in water) I put a sprig of mint. This was about the beginning of August, 1771, and after eight or nine days, I found that a mouse lived perfectly well in that part of the air, in which the sprig of mint had grown, but died the moment it was put into the other part of the same original quantity of air; and which I had kept in the very same exposure, but without any plant growing in it.

This experiment I have several times repeated; sometimes using air in which animals had breathed and died; and at other times using air tainted with vegetable or animal putrefaction; and generally with the same success.

Once, I let a mouse live and die in a quantity of air which had been noxious, but which had been restored by this process, and it lived nearly as long as I conjectured it might have done in an equal quantity of fresh air; but this is so exceedingly various,

various, that it is not easy to form any judgment from it; and in this case the symptom of *difficult respiration* seemed to begin earlier than it would have done in common air.

Since the plants that I made use of manifestly grow and thrive in putrid air; since putrid matter is well known to afford proper nourishment for the roots of plants; and since it is likewise certain that they receive nourishment by their leaves as well as by their roots, it seems to be exceedingly probable, that the putrid effluvium is in some measure extracted from the air, by means of the leaves of plants; and therefore that they render the remainder more fit for respiration.

Towards the end of the year some experiments of this kind did not answer so well as they had done before, and I had instances of the relapsing of this restored air to its former noxious state. I therefore suspended my judgment concerning the efficacy of plants to restore this kind of noxious air, till I should have an opportunity of repeating my experiments, and giving more attention to them. Accordingly I resumed the experiments in the summer of the year 1772, when I presently had the most indisputable proof of the restoration of putrid air by vegetation; and as the fact is of some importance, and the subsequent variation in the state of this kind of air is a little remarkable, I think

think it necessary to relate some of the facts pretty circumstantially.

The air, on which I made the first experiments, was rendered exceedingly noxious by mice dying in it on the 20th of June. Into a jar nearly filled with one part of this air, I put a sprig of mint, while I kept another part of it in a phial, in the same exposure; and on the 27th of the same month, and not before, I made a trial of them, by introducing a mouse into a glass vessel, containing two ounce measures and a half, filled with each kind of air; and I noted the following facts.

When the vessel was filled with the air in which the mint had grown, a very large mouse lived five minutes in it, before it began to shew any sign of uneasiness. I then took it out, and found it to be as strong and vigorous as when it was first put in; whereas in that air which had been kept in the phial only, without a plant growing in it, a younger mouse continued not longer than two or three seconds, and was taken out quite dead. It never breathed after, and was immediately motionless. After half an hour, in which time the larger mouse (which I had kept alive, that the experiment might be made on both the kinds of air with the very same animal) would have been sufficiently recruited, supposing it to have received any injury by the former experiment, was put into the same vessel of
air;

air ; but though it was withdrawn again, after being in it hardly one second, it was recovered with difficulty, not being able to stir from the place for near a minute. After two days, I put the same mouse into an equal quantity of common air, and observed that it continued seven minutes without any sign of uneasiness ; and being very uneasy after three minutes longer, I took it out. Upon the whole, I concluded that the restored air wanted about one fourth of being as wholesome as common air. The same thing also appeared when I applied the test of nitrous air.

In the seven days, in which the mint was growing in this jar of noxious air, three old shoots had extended themselves about three inches, and several new ones had made their appearance in the same time. Dr. Franklin and Sir John Pringle happened to be with me, when the plant had been three or four days in this state, and took notice of its vigorous vegetation, and remarkably healthy appearance in that confinement.

On the 30th of the same month, a mouse lived fourteen minutes, breathing naturally all the time, and without appearing to be much uneasy, till the last two minutes, in the vessel containing two ounce measures and a half of air which had been rendered noxious by mice breathing in it almost a year before, and which I found to be most highly noxious
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on the 19th of this month, a plant having grown in it, but not exceedingly well, these eleven days: on which account I had deferred making the trial so long. The restored air was affected by a mixture of nitrous air, almost as much as common air.

That plants are capable of perfectly restoring air injured by respiration, may, I think, be inferred with certainty from the perfect restoration, by this means, of air which had passed through my lungs, so that a candle would burn in it again, though it had extinguished flame before, and a part of the same original quantity of air still continued to do so. Of this one instance occurred in the year 1771; a sprig of mint having grown in a jar of this kind of air, from the 25th of July to the 17th of August following; and another trial I made, with the same success, the 7th of July, 1772, the plant having grown in it from the 29th of June preceding. In this case also I found that the effect was not owing to any virtue in the leaves of mint; for I kept them constantly changed in a quantity of this kind of air, for a considerable time, without making any sensible alteration in it.

These proofs of a partial restoration of air by plants in a state of vegetation, though in a confined and unnatural situation, cannot but render it highly probable, that the injury which is continually done
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to the atmosphere by the respiration of such a number of animals, and the putrefaction of such masses of both vegetable and animal matter, is, in part at least, repaired by the vegetable creation. And, notwithstanding the prodigious mass of air that is corrupted daily by the above-mentioned causes; yet, if we consider the immense profusion of vegetables upon the face of the earth, growing in places suited to their nature, and consequently at full liberty to exert all their powers, both inhaling and exhaling, it can hardly be thought, but that it may be a sufficient counterbalance to it, and that the remedy is adequate to the evil.

Dr. Franklin, who, as I have already observed, saw some of my plants in a very flourishing state, in highly noxious air, was pleased to express very great satisfaction with the result of the experiments. In his answer to the letter in which I informed him of it, he says,

“ That the vegetable creation should restore the
“ air which is spoiled by the animal part of it,
“ looks like a rational system, and seems to be of
“ a piece with the rest. Thus fire purifies water
“ all the world over. It purifies it by distillation,
“ when it raises it in vapours, and lets it fall in
“ rain; and farther still by filtration, when, keep-
“ ing it fluid, it suffers that rain to percolate the
“ earth. We knew before that putrid animal sub-
“ stances

stances were converted into sweet vegetables, when mixed with the earth, and applied as manure; and now, it seems, that the same putrid substances, mixed with the air, have a similar effect. The strong thriving state of your mint in putrid air seems to shew that the air is mended by taking something from it, and not by adding to it." He adds, "I hope this will give some check to the rage of destroying trees that grow near houses, which has accompanied our late improvements in gardening, from an opinion of their being unwholesome. I am certain, from long observation, that there is nothing unhealthy in the air of woods; for we Americans have every where our country habitations in the midst of woods, and no people on earth enjoy better health, or are more prolific."

May not plants also restore air diminished by putrefaction, by absorbing part of the phlogiston with which it is loaded? The greater part of a dry plant, as well as of a dry animal substance, consists of inflammable air, or something that is capable of being converted into inflammable air; and it seems to be as probable that this phlogistic matter may have been imbibed by the roots and leaves of plants, and afterwards incorporated into their substance, as that it is altogether produced by the power of vegetation. May not this phlogistic matter

matter be even the most essential part of the food and support of both vegetable and animal bodies?

Having discovered that vegetation restores, to a considerable degree of purity, air that had been injured by respiration or putrefaction, I conjectured that the phlogistic matter, absorbed by the water, might be imbibed by plants, as well as form other combinations with substances under the water. A curious fact, which has since been communicated to me, very much favours this supposition.

Mr. Garrick was so obliging as to give me the first intimation of it, and Mr. Walker, the ingenious author of a late English Dictionary, from whom he received the account, was pleased to take some pains in making farther inquiries into it for my use. He informed me that Mr. Bremner, who keeps a music-shop opposite to Somerset-house, was at Harwich, waiting for the packet; and observed that a reservoir at the principal inn was very foul on the sides. This made him ask the inn-keeper why he did not clean it out; who immediately answered, that he had done so once, but would not any more; for that after cleansing the reservoir, the water which was caught in it grew fetid, and unfit for use; and that it did not recover its sweetness till the sides and bottom of the reservoir grew very foul again. Mr. Walker ques-
tioned

tioned Mr. Bremner, whether there were any vegetables growing at the sides and bottom of it; but of this he could not be positive. However, as he said it was covered with a *green substance*, which is known to be vegetable matter (and indeed nothing else could well adhere to the *sides*, as well as to the bottom of the reservoir) I think it will be deemed probable, that it was this vegetating matter that preserved the water sweet, imbibing the phlogistic matter that was discharged in its tendency to putrefaction.

I shall be happy, if the mention of this fact should excite an attention to things of this nature. Trifling as they seem to be, they have, in a philosophical view, the greatest dignity and importance; serving to explain some of the most striking phenomena in nature, respecting the general plan and constitution of the system, and the relation that one part of it bears to another.

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SECTION III.

*Experiments of Plants growing in nitrated Air in the
Year 1777.*

HAVING heard that several persons abroad had not been able to repeat my experiments with the same success, I resumed them; and when I had made some progress in them, I heard of the experiments of Mr. Scheele on beans, who reports the result of them to have been constantly the reverse of mine. On this account I gave the more attention to this business in the spring and summer of 1778; and though I was interrupted in the prosecution of them, I made a considerable number in the beginning of the summer, the result of which was as follows.

1. In general, the experiments of this year were unfavourable to my former hypothesis. For whether I made the experiments with air injured by respiration, the burning of candles, or any other phlogistic process, it did not grow better but worse; and the longer the plants continued in the air, the more phlogisticated it was. I also tried a great variety

of plants, but with no better success, as sprigs of mint, spinach, lettuce, onions, brook-lime, and some others. The method in which I used them was, generally, to put the roots into phials filled with earth and water, and then to introduce them through water into the jar containing the air on which I was making the experiment; the jars being about ten inches in length, and two and a half in diameter.

2. I have had several instances of the air being undoubtedly meliorated by this process, especially by the shoots of strawberries, and some other plants, which I could, by bending, introduce into the jars or phials of air, supported near them in the garden, while the roots continued in the earth. This I thought to be the fairest method of trial, the plant growing, in every respect, in its natural way, except that part of the stem was obliged to lie in water, and the shoot was in air, confined in a narrow jar.

3. I had other instances, no less unquestionable, of common air not only receiving no injury, but even considerable advantage from the process; having been rendered in some measure dephlogisticated by it, so as to be much more diminished by nitrous air than before; a thing which I was far from expecting; having had nothing farther in view than simply to try whether the air would be injured or not;

not; Mr. Scheele, who made his experiments with beans, having always found it injured.

4. In most of the cases in which the plants failed to meliorate the air, they were either manifestly sickly, or at least did not grow and thrive, as they did most remarkably in my first experiments at Leeds; the reason of which I cannot discover. Indeed, I did not at this time make use of any air tainted with putrefaction, contenting myself with that which was injured by my own respiration, or the burning of candles; and it was in air tainted with the putrefaction of animal substances that my plants had flourished the most.

Upon the whole, I still thought it *probable*, from the experiments of this year, that the vegetation of healthy plants, growing in situations natural to them, has a salutary effect on the air in which they grow. For one clear instance of the melioration of air in these circumstances should weigh against a hundred cases in which the air is made worse by it, both on account of the many disadvantages under which all plants labour, in the circumstances in which these experiments must be made, as well as the great attention, and many precautions, that are requisite in conducting such a process.

SECTION IV.

Of the Growth of Plants in dephlogistigated Air.

SINCE air that has been injured by respiration or putrefaction is favourable to the growth of plants, it was natural to conclude, that dephlogistigated air must be unfavourable to them. But it is remarkable that plants will live tolerably well in very different kinds of air, even in inflammable air. However, in order to form some general idea of the effect of this pure kind of air in this respect, I made the following experiments. On the 10th of September, 1776, I took two sprigs of mint; and having put each of them into a phial of rain water, introduced one of them into a jar of dephlogistigated air, leaving the other in a jar of the same size, and with all other circumstances similar to it, in common air.

For some time I could perceive no difference between them, and neglected to take notice of them, till the 10th of October following; when I found the plant in the dephlogistigated air quite dead and black, and the other partially so, but the uppermost

most leaves were still alive. The dephlogisticated air was diminished one seventh of its bulk, and the other half as much.

This being late in the year, when the power of vegetation is languid, and being a single experiment, no general conclusion could be drawn from it.

But I was pretty well satisfied with respect to it, from experiments begun in April, 1777, and continued occasionally in the course of the summer following.

In order to compare the vegetation of plants in air differing as much as possible with respect to phlogiston, I took three sprigs of mint; and having put all their roots into phials containing the same pump water, that had been some time exposed to the open air, I introduced one of them into a jar of common air, another into one of dephlogisticated air, and the third into air that had been phlogisticated with nitrous air several months before. It was in such a state, that one measure of it and one of nitrous air occupied the space of 1.75 measures. This was done in April; and examining the plants on the 12th of May following, I found that the plant in this phlogisticated air had grown remarkably well, much better than that in the common air; whereas the plant in the dephlogisticated air had a very sickly appearance.

I examined these plants on the 26th of the same month, when the appearances continued nearly the same, and then, examining the state of the air, I found that, though the plant in phlogisticated air had grown so well, the air was not sensibly improved by it. The dephlogisticated air was injured, which I attributed to the rotting of some of the leaves of the plant. The common air I did not attend to.

On the 7th of June following, I took an account of three sprigs of mint, which had been growing, I believe, some weeks in dephlogisticated air, and of three others, which had been growing the same time, and in all the same circumstances in other respects, in common air; and observed that, in all the three cases, the appearances were decisively in favour of the plants in the common air, the shoots being twice as large, and every other appearance of health in the same proportion.

I do not say that even these observations are quite sufficient to determine the question; but they seem to make it *probable*, that dephlogisticated air does not supply that pabulum which plants derive even from common air; though I own it may injure them on some other account. Even Mr. Scheele, who maintains that vegetation has the same effect on air that respiration has, I find, allows that plants do not grow so well in dephlogisticated as in common air.

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SECTION V.

Of the State of Air confined in the Bladders of Sea Weed.

I WAS much confirmed in the hypothesis of vegetation restoring atmospherical air to a state of greater purity, by finding the air within the bladders of the common *sea weed* to be considerably better than the common external air. This was a casual and unexpected observation that I made in the course of a summer which I spent at Lymington, and I wish that some philosophical persons who live near the sea would examine this circumstance a little farther, both for the sake of investigating the *origin* of this air, and the particular oeconomy of the plant that contains it. It might even lead to some farther knowledge of the structure of plants in general.

Before I recite these observations, I would remind my reader, that I formerly gave some attention to the air contained in the hollow parts of certain plants, particularly the *bladder sena*, and the stalks of onions; but, in those two cases, I found

the air, as far as I could then judge, not to differ from that of the surrounding atmosphere. This being an observation of no consequence, I desisted from prosecuting it, imagining there must be some easy communication between those cavities in plants and the external air, so that much difference could not be expected. I found, however, in the course of this summer, that, in two other cases, air so confined was much inferior in purity to that of common air.

Air pressed out of the stalks of the common *flag* (as I think it is called) growing in water, was in such a state, that one measure of it and one of nitrous air occupied the space of 1.5 measures. And air in the inside of a plant resembling hemlock was even worse than this. For when I examined it I found the measures of the test to be 1.75.

Upon this I was rather inclined to suppose, that if the air within the cavities of plants was examined with rigour, it would always be found rather worse than the air in the surrounding atmosphere, especially if the plant was in the smallest degree unhealthy; as the phlogiston discharged in any tendency to disease would easily affect the air of such cavities; and there being no visible circulation, it would probably retain such a taint a considerable time. Though I might have supposed, that if the plant was very healthy, and did imbibe phlogiston
from

from the neighbouring air, the air in those cavities (in what manner soever it came there) would be *depurated* by that means, and thereby approach to the state of dephlogisticated air. This may perhaps be the case with the air in the bladders of sea weed, though I could wish to know a little more concerning the origin of this air. For as some of the plants grow intirely under water, there is no appearance of this air having ever been atmospherical air, but rather of its being generated within the plant itself.

I observed three kinds of this sea weed, one which I take to be the *quercus marinus*, the bladders, when full grown, being about half an inch in diameter, and rather of an oval form; another in which the bladders were spherical, about a quarter of an inch in diameter; and a third in which the bladders were much larger than these, being formed by the separation of two *laminae* of which the plant consists, so as to resemble a fillet, the bladder being exactly of the breadth of the flag, and rather longer than it is broad.

The first of these was most common on the sea shore at Lymington. The first that I took up had lain a considerable time on the shore, so that the bladders were become very hard and brittle, and the air within them was exactly in the same state with the air of the atmosphere. But afterwards, on the

25th of July, I happened to meet with a quantity of this weed that had just been thrown up by the sea, quite moist, and the bladders soft. Bursting them under water, and examining the air, I found that one measure of it and one of nitrous air occupied the space of not more than one measure; whereas, when I applied the same test to the common air, the measures were 1.3.

SECTION VI.

Of the spontaneous Emission of dephlogisticated Air from Water containing a vegetating green Matter.

FEW persons, I believe, have met with so much unexpected good success as myself in the course of my philosophical pursuits. My narrative will show that the first hints, at least, of almost every thing that I have discovered of much importance, have occurred to me in this manner. In looking for one thing I have generally found another, and sometimes a thing of much more value than that which I was in quest of. But none of these unexpected

expected discoveries appear to me to have been so extraordinary as that which I am about to relate; and it may serve to admonish all persons who are engaged in similar pursuits, not to overlook any circumstance relating to an experiment; but to keep their eyes open to every new appearance, and to give due attention to it, how inconsiderable soever it may seem.

In the course of my experiments on the growth of plants in water impregnated with fixed air, I observed that bubbles of air seemed to issue spontaneously from the stalks and roots of several of those which grew in the unimpregnated water; and I imagined that this air had percolated through the plant. It immediately occurred to me, that if this was the case, the state of that air might possibly help to determine what I was at that time investigating, viz. whether the growth of plants contributes to purify, or to contaminate the air. For if this air should prove to be better than common air, I thought it would show, that the phlogiston of the imbibed air had been retained in the plant, and had contributed to the nourishment of it, while that part of the air which passed through the plant, having deposited its phlogiston, had been rendered purer by that means; though if the air should not have been found better than common air, I should not have concluded my hypothesis was false; since
plants,

plants, like animals, might take in phlogiston in one state, and emit it in another.

With this view, however, I plunged many phials, containing sprigs of mint in water, laying them in such a manner, as that any air which might be discharged from the roots would be retained in the phials, the bottoms being a little elevated. In this position the sprigs of mint grew very well, and in some of the phials I observed a quantity of air to be collected, though very slowly; but I was much disappointed in finding that some of the most vigorous plants produced no air at all. At length, however, from about ten plants, I collected, in the course of a week, about half an ounce measure of air. This was the 19th of June, 1778; and, examining it with the greatest care, I found it so pure, that one measure of it and one of nitrous air occupied the space of only one measure.

This remarkable fact contributed not a little to confirm my faith in the hypothesis of the purification of the atmosphere by vegetation; but I did not enjoy this satisfaction long. For I considered that, if this was the proper effect of vegetation, it must be universal, and could not be confined to a few plants, especially when others of the same species produced no such effect. Besides, when I removed the air-producing plants, as I thought them to be, into other and cleaner phials, I found that they yielded

yielded no more air than the other plants had done. And, what I thought more extraordinary still, the phials in which these plants had grown, the insides of which were covered with a green kind of matter, continued to yield air as well when the plants were out of them, as they had done before. This convinced me that the plants had not, as I had imagined, contributed any thing to the production of this pure air.

About the same time I observed that great plenty of air rose spontaneously from the bottom and sides of a tall conical receiver, about eighteen inches high, and five wide at the bottom, originally made for the experiment of the fountain in vacuo, but which I had often used as a magazine for various kinds of air, and which was at that time employed for the same purpose; and both the plate on which it stood inverted, and the lower part of the receiver, were covered with this green matter.

To make my observations on this new subject of experiment with more attention, I transferred the air it had contained into another vessel, filled the receiver with fresh pump water, and placed it where it had stood before, which was in a window on which the sun shone; when air bubbles presently began to rise very fast, so that, in three days, I had collected seven ounce measures, and this was so pure,
that

that one measure of it, and two of nitrous air occupied the space of four fifths of a measure.

Having found many of my phials which had the same green matter in them, I filled them also with fresh pump water ; and, inverting them, I collected from them all considerable quantities of the same dephlogisticated air, especially when they were placed in the sun ; and it was very amusing to watch them, and to observe the bubbles swell, and detach themselves gradually from the green matter.

When I had advanced thus far in this interesting inquiry, I was obliged to desist from the farther prosecution of it, on account of a journey, on which I was absent some months ; and all that I could do was to leave a number of phials filled with different kinds of water, as river water, pump water, and rain water, with several other little varieties, in order to discover the circumstances that were most favourable to the production of this green matter, whatever it was.

At my return, on the 8th of September, I found no green matter in any of the phials, excepting those which contained pump water. Neither the rain water, or river water, had produced any. This pump water contains a considerable quantity of fixed air, and I must also observe that the insides of the middle and lower glasses in one of Mr. Par-

ker's apparatus's for impregnating water with fixed air were almost coated with this green matter.

After this I placed in my garden a large glass jar nearly filled with pump water, which I had strongly impregnated with fixed air, and also jars of river water, rain water, and pump water unimpregnated; and on the 14th of October, I found almost all the bottom of the jar which contained the impregnated water covered with the green matter, but there was none at all in any of the other jars. This made it probable, that the fixed air in the water contributes to the production of this matter.

That the external air, or animalcules in it, have nothing to do in the formation of this green matter, is evident from several of the preceding observations. This could not be the case, for instance, with the large inverted receiver, which had always yielded the greatest quantity of this air, or with the water in the middle vessel of Mr. Parker's apparatus. Besides, at other times I have kept phials closely corked, and yet have found the green matter at the bottom of them, and it has yielded air plentifully, especially in the sun, or when placed near the fire. For when the matter is once formed, nothing but a certain degree of warmth seems to be necessary to its actual production of air.

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The production of this green matter in close vessels seemed to prove that it can neither be of an animal or a vegetable nature, but a thing *sui generis*, and which ought, therefore, to be characterized by some peculiar name; and all the observations that I made upon it with the microscope agreed with this supposition. For, excepting a few filaments, that were hollow, and two or three globular pieces, perforated with some regularity, all the rest of the substance seemed to be a congeries of matter of a compact earthy nature, the pieces separately taken resembling bits of jelly.

I had some appearances very early, which, extraordinary as I thought it, made it probable that *light* was necessary to the formation of this substance. On the 23d of October, I observed that two small phials, which had been filled with pump water, and closely corked on the 9th of August preceding, had both of them a quantity of this green matter, while an open jar of the same water, but in a much worse light had none of it. There was, indeed, a greater depth of water in the jar than in the phials; and though I had generally observed that this green matter is first formed at the bottom of the vessels, it may possibly require more time to the formation of it in proportion to the depth of water. Two other jars, however, about an inch

deeper than that above-mentioned, and quite filled with the same water, placed in the window on which the sun shone had acquired this green matter, even in less time than the two small phials above-mentioned.

From *green*, this substance passes gradually to a kind of *yellow*, or rather *orange colour*. For on the 14th of October, I observed that the large receiver in which I had at first collected a considerable quantity of this pure air, and which I had always kept full of water, continued to yield air as copiously as ever, though both on the receiver itself, and on the plate on which it stood, the colour of this substance was quite changed to the orange colour above-mentioned.

On the 17th of September I had taken all the air from this receiver, and on the 14th of October following, on which I observed its change of colour, I took from it about nine ounce measures of air, the very purest that I had ever got in this method. For one measure of it and two of nitrous air, occupied the space of 0.44, which is quite as pure as dephlogisticated air at a medium.

Observing one of my phials of water that had got a coating of the green matter yielding air very copiously, I poured the water out of it into a clean phial, and found that, by the agitation given to it in the act of decanting, it sparkled as much as any

Prymont or Seltzer water. Inverting it in a bason of water, I collected the air, and found it to be very pure. I treated several other phials in the same manner, and the subsequent appearances being the same, I had no doubt but that when water is brought into a state proper for depositing that green matter, it is, by the same process, prepared for the spontaneous emission of a considerable quantity of pure air.

I never found it except in circumstances in which the water had been exposed to *light*; and when, after standing in the dark, the water has deposited a whitish filmy matter, it has become green after a few days exposure to the sun. It was most freely deposited from my pump water, and especially when it had been impregnated with fixed air, but I have found it both in river water, and rain water, after longer standing. I have generally found it at the bottom of the vessel, but sometimes it has been first formed at the top, and the coating from the bottom and that from the top meeting, the whole phial has acquired a coating of it from being once filled with pump water.

When I have kept water a long time in the shade, it has not generally yielded any other kind of air than it would have yielded at the first; and, though, when it has been kept in an open vessel, the air has been better, it has never been so good as the air

air in the same kind of water that has been exposed a much less time to the sun.

No degree of *warmth* will supply the place of the sun's light; and though, when the water is once prepared by exposure to the sun, warmth will suffice to *expel* that air; yet, in this case, the air has never been so pure, as that which has been yielded spontaneously, without additional heat. The reason of this may be that, besides the air already depurated, and on that account ready to quit its union with the water, heat expels, together with it, the air that was phlogisticated, and held in a closer union with the water.

The large receiver of which I have made mention, as having served me for a magazine of air, and which I find contains 135 ounces of water, I have already observed yielded, when filled with pump water, nine ounce measures of very pure dephlogisticated air, after being exposed to the sun from the 17th of September to the 14th of October. I then filled it with fresh pump water, and placed it in my laboratory till the 8th of December (with its mouth inverted, as before, in a dish of the same water) and in all this time not a single bubble of air came from it. But on being placed in a south window it immediately began to yield air, and continued so to do whenever the sun shone, till the 21st

of January following, when there might be about four ounce measures of air in it. I then placed the receiver, and the dish in which it stood, in a large pan of water, which I made to boil, and kept boiling the whole day, till no more air could be discharged from it; and the next morning, when it was cold, I examined the air, and found it to be in all six ounce measures. No part of it was fixed air, and one measure of it and two of nitrous air occupied the space of .9 measures; whereas, with the air produced spontaneously, in the light of the sun, the same measures were .44, and the quantity nine ounce measures.

SEC.

SECTION VII.

Of the Purification of Air by Plants and the Influence of Light on that Process.

ONE of my earliest observations on the subject of air, but made casually, when, in fact, I expected a contrary result from the process, was the purification of air injured by respiration or putrefaction, by the vegetation of plants. But at that time I was altogether ignorant of the part that *light* had to act in the business. At the publication of the experiments recited in the last section, I had fully ascertained the influence of light in the production of dephlogisticated air in water by means of a *green substance*, which I at first supposed to be a plant, but not being able to discover the form of one, I contented myself with calling it simply *green matter*.

Several of my friends, however, better skilled in botany than myself, never entertained any doubt of its being a plant; and I had afterwards the fullest conviction that it must be one. Mr. Bewly has lately observed the regular form of it by a microscope. My own eyes having always been weak, I

have, as much as possible, avoided the use of a microscope.

The principal reason that made me question whether this green matter was a plant, besides my not being able to discover the form of it, was its being produced, as I then thought, in a phial close stopped. But this being only with a common cork, the seeds of this plant, which must float invisibly in the air, might have insinuated themselves through some unperceived fracture in it; or the seeds might have been contained in the water previous to its being put into the phial. Both Mr. Bewly and myself found, in the course of the last summer, that when distilled water was exposed to the sun, in phials filled in part with quicksilver, and in part with distilled water, and inverted in basons of quicksilver, none of this green matter was ever produced; no seed of this plant having been able to penetrate through the mercury, to reach the water incumbent upon it, though, in several cases, it will be seen, that these seeds diffuse and insinuate themselves, in a manner that is truly wonderful.

Without light, it is well known, that no plant can thrive; and if it do grow at all in the dark, it is always white, and is, in all other respects, in a weak and sickly state. Healthy plants are probably in a state similar to that of *sleep* in the absence of light, and do not resume their proper functions, but by the

the influence of light, and especially the action of the rays of the sun. This was the reason why no green matter was ever produced by means of mere *warmth* in my former experiments, and that in jars standing in the same exposure, but covered, so that the light had no access to them, no pure air was collected, none of the green matter being then found in them.

This I verified most completely by covering the greatest part of a glass jar with black sealing-wax, which made it thoroughly opaque; and besides answering that purpose better than brown paper, as I made the experiment before, did not imbibe any of the water, and therefore did not promote the evaporation of it. To be able to observe whether any air was collected in these jars, or not, the upper part of them was not coated with sealing wax, but had a thick moveable cap of paper, which I could easily take off, and then inspect the surface of the water.

In order to satisfy myself as fully as possible with respect to this remarkable circumstance, I also made the following experiments, the result of which are, indeed, very decisive in favour of the influence of *light* in this case.

Having a large trough of water, full of recent green matter, giving air very copiously, so that all the surface of it was covered with froth, and jars filled with it, and inverted, collected great quan-

ties of it, and very fast ; I filled a jar with it, and, inverting it in a basin of the same, I placed it in a dark room. From that instant no more air was yielded by it, and in a few days it had a very offensive smell, the green vegetable matter with which it abounded being then all dead, and putrid.

Again, having filled a receiver with fresh pump water, and having waited till it was in a state of giving air copiously, I removed it into a dark room ; and from that time the production of air from it intirely ceased. When I placed it again in the sun, it gave no air till about ten days after, when it had more green matter, the former plants being probably all dead ; and no air could be produced till new ones were formed.

With the same view I placed some small slices of *roasted beef* in a vessel of water in the sun, and an equal quantity, in another vessel of the same size, in the dark ; when the former became green, and yielded air, but the latter not at all. It will be seen afterwards that many animal substances afford an excellent pabulum for this green vegetable matter.

I also made a similar experiment with slices of *cucumber*, when those in the sun became covered with green matter, and yielded pure air, but those that had been placed in the shade, though they did yield a small quantity of air, it was wholly phlogisticated,

gisticated, though not inflammable, which many vegetable substances yield in the same circumstances.

That it was the *green matter* that yielded the air, and not the mere action of *light* upon the water, might be inferred from my former experiments; and this was my own idea at the first, though I quit-
ted it afterwards. The appearance which then misled me was the great quantity of pure air emitted by the water, after it was poured off from the green matter. But before any air can appear on the surface of the water, in its elastic state, the water itself must be thoroughly saturated with it, in which case it contains so much air, that, upon the least agitation, even without heat, it readily parts with it, and exhibits the beautiful appearance which I then described. But that, notwithstanding this appearance, it was the *green matter*, and not the *water* that yielded the air, I was convinced by the following experiment.

Having a number of earthen plates covered with green matter, I introduced several of them under vessels filled with fresh pump water, and then placed them in the sun, together with other vessels filled with the same water, at the same time, but standing on clean plates; when I constantly found that air was immediately produced in the vessels containing the green matter but none in the others,
till

till the green matter was naturally formed in them; after which, but not before, pure air was produced in those vessels also.

I likewise used water that had long been exposed to the sun's light, so that it must have deposited every thing that mere *light* could make it part with; and yet in this water, upon plates of green matter, air was immediately produced, as well as in water that had never been exposed to the sun.

I was led to these experiments by observing that air was immediately produced from those parts of my jars to which green matter from former experiments happened to adhere, not having been carefully cleaned. It was likewise an evidence that it was the green matter, and also in a vegetating state, which yielded the air, that when a plate covered with it had been made pretty hot before the fire (by which the plants had probably been killed) it was incapable of yielding any air.

Having, by this means, fully satisfied myself, that the pure air I had procured was not from the *water*, but from the green vegetating substance assisted by light, I concluded that other *aquatic plants* must have the same effect; and going to a piece of stagnant water, the bottom of which was covered with such plants, I took five or six different kinds promiscuously. Then, having put them into separate jars of the water in which they were growing,

ing, and inverted them in basons of the same, I placed them in the sun; and I found that all of them, without exception, were immediately covered with bubbles of air, which gradually detaching themselves from the leaves and stalks, where they had originated, rose to the surface of the water; and this air, being examined, appeared to be, in all the cases, very pure, though not quite so pure as that which I had before procured from the green matter; the measures of the test, with two equal quantities of nitrous air, being, at a medium, 1.5. Afterwards air procured from these plants was very nearly as pure as the other.

In order to ascertain with more precision the real origin of this pure air, and especially to determine whether it was properly *produced* by the light, and something within the plant (which, as I found afterwards, seems to be the idea of Dr. Ingenhousz*) or only by dephlogisticating the air pre-

* He says (Experiments on Vegetables, p. 23) that "the air obtained from the leaves is by no means air from the water, but air continuing to be produced by a special operation carried on in a living leaf, exposed to the day light, and forming bubbles, because the surrounding water prevents this air from being diffused through the atmosphere." Again, p. 89, he says of the green vegetable matter, "It is wonderful that this green matter seems never to be exhausted of yielding dephlogisticated air, though it has no free communication with the common atmosphere, from which the most part of other plants seem to derive their stock of air. Does this vegetable matter imbibe this air from the water,

viously contained in the water, which I suspected from my former experiments on vegetation, I kept a quantity of these water plants in jars of water in the sun, as long as they would give any air; then only changing the water I found that the same plants immediately began to give fresh air as copiously as at first. The particulars of the experiment were as follows.

I put a handful of these water plants, without distinguishing their kinds, into a receiver containing eighty ounce measures of water, inverted in a basin of the same; and when they had yielded between six and seven ounce measures of air, I examined it, and found that with two equal quantities of nitrous air, the measures of the test were 0.8. But the air had been diminishing about three days, so that I believe there had been eight ounce measures in all, or one tenth of the capacity of the jar, and certainly purer than it was now found to be. It was evident, therefore, that no more air would have been produced by these plants in this water, though placed in the sun. Replacing this jar with

“and change it into dephlogisticated air? this does not seem to
“me probable—I should rather incline to believe that the wonder-
“ful power of nature, of changing one substance into another, and
“of promoting perpetually that transmutation of substances which
“we may observe every where, is carried on in this green vegetable
“matter in a more ample and conspicuous way.”

more

more of the same river water, the same plants were instantly covered with air bubbles, and in a very few hours had yielded more than an ounce measure of air. Some *duck weed*, which swam on the top of the water, in the former part of this experiment, was dead, owing, no doubt, to the purity of the air to which it had been exposed.

To conclude this series of experiments, I expelled air from a quantity of this river water, both before the plants were put into it, and afterwards; and I found that the air contained in it was purer after the plants had been confined in it than before, though the whole piece of water being quite full of plants, the air contained in it was tolerably pure in the first instance. The measures of the test, with an equal quantity of nitrous air being 1. But the air expelled from the water after the plants would grow in it no longer, was so pure, that with ~~two~~ equal quantities of nitrous air, the measures of the test were 1. Also, whereas the phial of water in the first case gave only 2.4 ounce measures of air, the same phial afterwards gave 4.4; which is nearly twice as much. For, as I have observed before, whereas the phlogistication of air diminishes the quantity of it, the dephlogistication must increase the quantity; and this increase exceeding the quantity which the water is capable of holding in solution,

tion, part of it is detached, and appears in an elastic form on the surface.

It is also a proof that the proper origin of all the air produced in these circumstances is not the plant and the light, and that these are only agents to produce that effect upon something else, that in all cases, the quantity of air produced bears a certain general proportion to the capacity of the vessel in which the process is made, never, I believe exceeding one eighth, exclusive of that which is held in solution by the water itself, which, however, is pretty considerable.

A jar containing one hundred and fifteen ounce measures was filled with pump water the 2d of June, and it presently began to yield air, and continued to do so about a fortnight, after which very little was produced. The quantity I received from it was twelve ounce measures, which is more than one tenth of the bulk of the water, and as highly dephlogisticated as almost any that I had ever procured. The reason why this jar began to yield air immediately was, that green matter from former experiments adhered to several parts of it.

At another time I observed, that when a large earthen trough, filled with pump water, was very turbid, with green matter floating in it, and in a state of giving air very plentifully, if I inverted any
jar

jar full of it, it would, in about a week, yield one eighth of its contents of air; and examining it, I found this air so pure, that with two equal quantities of nitrous air, the measures of the test were 0.5.

From this experiment it was very evident, that there is no proper *production* of air in the case, but only a *depuration*, or dephlogistication, of the air previously contained in the water; and as water plants depurate the air that is held in solution by the water, it is agreeable to analogy that plants growing in air should depurate that air to which they are exposed. This led me to try whether plants growing in air would, when wholly immersed in water, though it be not their natural element, exert, and retain for any time, their power of depurating air. But still, to keep as near as I could to the water plants, with which I had had so much success, I pitched upon the *water flag* for the experiment; the root of this plant and part of the stalk being in water; though the upper part emerges out of it. Not suspecting that the mere *leaves* of a plant retain so much life as Dr. Ingenhoufz found them to do (and as I might have learned from Mr. Bonnet) I took three whole plants, and put them into tall jars of water for the purpose; when I observed that the leaves were presently covered with air bubbles, and continued to give air the whole day. This
air

air I observed to stream from both sides of the leaf, and every part of the stalk. This air I examined, and found it, in one case, to be something worse than common air, but in another case, something better, though not considerably so. Before I proceeded to make trial of any other plants, I was informed of the experiments of Dr. Ingenhoufz, whose assiduous attention to this subject gave me the greatest satisfaction, and entirely superseded what I might otherwise have thought of doing in the same way.

It appears from these experiments, that air combined with water is liable to be phlogisticated by respiration, and to be dephlogisticated by vegetation, as much as air in an elastic state, out of water. For fishes, as I shall observe, foul the air contained in the water in which they are confined, and water plants now appear to purify it. This is, no doubt, one of the great uses of weeds, and other aquatic plants, with which fresh water lakes, and even seas abound, as well as their serving for food to a great number of fishes.

The experiments recited in this section may help us to explain why water, after issuing from the earth, and employed in floating meadow land, becomes in time exhausted of its power of fertilizing them. When it issues from the earth, it contains air of an impure kind; that is, air loaded with phlogiston.

gifton. This principle the roots of the grass extract from it; so that it is then replete with dephlogisticated air, and consequently the plants it afterwards comes into contact with, find nothing in it to feed upon.—I believe it is commonly imagined, that the water deposits something in its course upon the earth of its bed, and by that means becomes effete, and incapable of nourishing plants.

I therefore desired Mr. Young, to whom this country owes so much for the attention that he has given to the business of agriculture, to send me a quantity of such water as had been found to be most useful in fertilizing meadows. He was so obliging as to send me two bottles of such water; and as soon as I received them I expelled from them the air they contained, and found that which was in one of them, sufficiently impure, the standard of it being 1.4; but that in the other, was such as is usually contained in water that has been long exposed to the atmosphere, being of the standard of 1.0; so that I imagined it had not been sufficiently well corked. After the water in the other bottle had been exposed a few days to the open air, the air contained in it was of the same quality. Upon the whole, I think that the experiment was sufficiently favourable to the hypothesis, but more ought to be made, and if possible, on the spot.

SECTION VIII.

Farther Observations on the green vegetable Matter with which many of the preceding Experiments were made.

I Very much doubt whether the green matter, which was the subject of the preceding experiments, has ever been properly noticed by botanists. The *conserua fontinalis*, as it is described by Dr. Withering, in his most useful system of botany, though said to have threads *extremely short*, is only said to have them "sometimes not more than half an inch in length," and it is also said to be of a *brownish green*. Whereas this whole plant cannot be one tenth of an inch in length, and it is of a beautiful lively green. It will be thought, however, I imagine, to come most properly under the denomination of the *conserua*; but this not being within my province, I shall not presume to give it any particular appellation, though I might be inclined to call it *the water moss*. I shall therefore continue to call it, in general, the *green matter*, or the *green vegetable substance*. Whether this plant has ever got a name in systems of botany, or not,

its

its *natural history* is certainly unknown; and therefore, in this and the following section, I shall give such an account of its mode of growth, and other particulars relating to it, as have happened to fall under my observation*.

In general the seeds of this plant (for I presume that, like other plants, this also must have seeds) float invisibly in the air, and are capable of producing plants in all seasons of the year, whenever they meet with water, especially if it be impregnated with not too great a proportion of vegetable or animal substance, in a state of putrefaction; and if it does not actually freeze, the plants never fail to appear in their vigour, so as to be capable of producing air, in the space of a few days. But though the richest pabulum for this plant is the

* Dr. Ingenhoufz's idea of the origin of this vegetable matter, as he himself allows it to be, is rather extraordinary, considering how long the doctrine of *equivocal*, or *spontaneous generation*, has been exploded. He says, p. 90, "The water itself, or some substance in the water, is, as I think, changed into this vegetation, and undergoes, by the influence of the sun shining upon it, in this very substance, or kind of plants, such a metamorphosis, as to become what we now call dephlogisticated air.—This real transmutation, though wonderful in the eye of a philosopher, yet is no more extraordinary than the change of grass and other vegetables into fat, in the body of a graminivorous animal, and the production of oil from the watery juice of an olive tree." But the change of *water* into an *organized plant*, is a thing of a very different nature from these.

putrescent parts of animal and vegetable substances, some of them are unfavourable to it, and prevent its growth.

The seeds of this plant insinuate themselves into vessels of water through the smallest apertures, and then diffuse themselves through the whole mass of it; so that when the largest jars are filled with water, and placed inverted in basons of the same, and consequently the seeds must enter between the basons and the bottoms of the jars, the plants will first appear at the very top of the jar, if the best pabulum for it be lodged there. It will likewise appear from the following experiments, that, though the tendency to produce pure air is favoured by a certain quantity of putrefactive matter in the water in which these plants are found, the quantity may be so great, as to counteract the operation of the plant, and phlogisticate, and diminish, the air as fast as it is produced.

As I shall generally describe the whole of every process, just as I noted the appearances at the time, the necessary *influence of light* in the production of dephlogisticated air, as well as other circumstances already proved by the experiments recited above, will occasionally receive additional confirmation.

I have found a slower and a less produce of air from rain water than from pump water; owing, I suppose, to the rain water containing less air to operate

operate upon, and generally also in a purer state than that which is contained in pump water.

On the 8th of June, I placed, in the open air, a large jar of rain water, inverted in a basin of the same; but no green matter appeared in it before the 22d of the month. On the 24th of July, finding no more air produced, I examined it, and found the quantity to be two ounce measures and a half: and with two equal quantities of nitrous air, the measures of the test were 1.24. This rain water, which was received in a large tub from the roof of a house, yielding so little air of itself, I generally made use of it when I tried the effect of different impregnations of water.

The green matter, and consequently the production of air, also generally appeared very late in *distilled water*, which is a confirmation of the hypothesis mentioned above. For water after distillation must have time to imbibe air from the common atmosphere for this plant to operate upon, before any air can be produced from it. On this account, I have always found that this effect has been soonest produced in the smallest vessels. Having, on the 20th of August, exposed to the air a jar nine inches deep, and another of four inches, not inverted in basins, but simply filled with distilled water, the latter was covered with green matter on the 6th of September; whereas it did not

appear

X 3

appear

appear in the taller vessel till a considerable time afterwards.

In one experiment (from which, however, I would not draw any certain conclusion) distilled water was more favourable to the production of this green matter than rain water, which, being collected from the roof of a house, might contain some peculiar impregnation unfavourable to vegetation.

I put four pennyweights of boiled mutton into the belly of a retort, containing about a pint, filled with distilled water, and an equal quantity of the same mutton I put into a retort of the same size, filled with rain water; and observing them nine days afterwards, I found the latter of a reddish hue, with very little air, whereas the former was all green, and in a state of yielding air very copiously. The mouth of this retort was immersed in a vessel of water seven inches deep, and was also closed with a cork, which had a very small perforation in it, in order to cut off, as much as possible, all communication with the external air. Perhaps the seeds of this plant might have been in this water previous to its exposure, though it had been distilled not long before the experiment.

I found this green matter in a state of giving air in water, which had formerly been impregnated with fixed air and iron. The fixed air, however, being gone, and the iron precipitated, nothing but
simple

simple water was left. But I likewise found this plant in water impregnated both with *common salt*, and with *saltpetre*, which impregnations water will not part with in the open air.

The water was impregnated with common salt, so as to make it about the same degree of saltiness with that of sea water, and it was exposed in a tube an inch in diameter, and three feet long, inverted in a pot of the same water. All the inside of the tube was in time nearly covered with small green knobs, almost contiguous to each other, and not with such an uniform coating as is generally found in common water. The air was very pure. De-phlogisticated air was also procured at the same time in a similar tube, filled in the same manner, with water impregnated with an equal quantity of *nitre*. But the air in this appeared not to be quite so pure as that in the water impregnated with common salt.

Having impregnated a quantity of water very strongly with fixed air, I placed it in an inverted phial, and observed that no green matter appeared in it of a long time; but when the fixed air might be supposed to have made its escape, the green matter appeared; and the air, when examined, was found to be of the purest kind, without the least mixture of fixed air in it. With two equal quan-

ties of nitrous air, the measures of the test were 0.5.

In order to observe on what part of a vessel of water the seeds of this plant would first fall, and in what manner they would then propagate themselves, I placed in the sun a glass tube one inch wide and three feet long, in an inclined position, but with its mouth upwards, filled with distilled water. The green matter first appeared, in small specks, about two inches below the surface of the water, on the side to which it was inclined, then on the same side near the middle of the tube, and lastly all the bottom was covered with it. On the whole, the tube presented the appearance of the seeds having been let fall into it perpendicularly, and passing through the water to have fixed themselves where they happened to impinge. Had the tube been placed perpendicularly, the green matter would, I suppose, have appeared first at the bottom of it, as indeed I have generally found to be the case, and would have extended itself from thence to the sides of the tube.

once whether they were placed in the sun or in the shade. Whereas other substances, which, if they had been confined by quicksilver, would have yielded, by putrefaction, inflammable air also, together

SECTION IX.

Of the Production of Green Matter, and of pure Air, by Means of various Vegetable Substances in Water.

HAVING very soon observed that this green vegetable matter, or *water moss*, was planted and propagated with more ease, and produced air more copiously, in some circumstances than in others, and that various substances, animal and vegetable, were favourable to it, and others of both kinds unfavourable, I tried a great variety of them, and shall recite such of the particulars as appear in any measure remarkable, and such as may furnish hints for the farther investigation of what relates to this subject.

The most remarkable circumstances attending these experiments was, that some substances, concerning which I could have had no such expectation a priori, instead of admitting the growth of this plant, when they began to putrify, and dissolve, which was the case with most vegetable and animal substances, yielded from themselves a very great quantity of inflammable air, and it made no differ-

ence

ence whether they were placed in the sun or in the shade. Whereas other substances, which, if they had been confined by quicksilver, would have yielded, by putrefaction, inflammable air also, together with a portion of fixed air, only supplied the proper pabulum for this green matter, and the whole produce was pure dephlogisticated air; the phlogiston, which in other circumstances would have been converted into inflammable air, now going to the nourishment of this plant, which by the influence of light yields such pure air.

I shall, in the first place, give an account of the experiments I made with the *leaves* of plants, and then with some other parts of them, confining myself chiefly to such as are commonly used for food; having in that choice a view to the *principle of nutrition*, besides that such substances were most at hand.

On the 18th of June, I put eighteen pennyweights of green *cabbage* into a large jar of rain water. On the 28th the water began to be a little turbid, and the vessel contained three ounce measures of air, no part of which was fixed air; and, with two equal quantities of nitrous air, the measures of the test were 1.44. Having changed the water, and left the cabbage in the same vessel, on the 18th of July there was in it six ounce measures of air, which was increasing very rapidly, all the water being very green;

green; but after the 19th, little more air was produced. At this time I collected ten ounce measures, no part of which was fixed air, and with two equal quantities of nitrous air, the measures of the test were 0.67. The cabbage was then soft, but not offensive.

Replacing the same cabbage in fresh water, on the 27th of July several ounce measures of air were produced, and on the 29th I took from it eight ounce measures, the production of air having ceased a day or two before. This air was quite as pure as the last; for the measures of the test were 0.6, and the cabbage was still soft, but not in the least offensive. The reason of this, I imagine, was, that the phlogiston, which would have constituted the offensive smell of the cabbage (and no putrid vegetable substance is more offensive) was, in this case, imbibed by this *water moss*, as fast as it was produced by the process of putrefaction; and the vessel being large, there was no superabundant phlogiston to contaminate the air.

In order to try what effect a larger quantity of cabbage in proportion to the size of the jar would have, and also what would be the difference of its putrefying in the *dark*, I made the following experiment.

On the 19th of July I put two ounce measures and a half of cabbage into a small vessel of water
in

in the sun, and in a similar vessel an equal quantity of the same cabbage in a dark room. On the 25th the water of the vessel in the sun had a whitish appearance, and about an ounce measure of air was produced; but at the same time there was a much larger quantity of air produced from the cabbage in the dark, the water being turbid also. The day following I examined the air from the dark room, and found it to be sixteen ounce measures, one third fixed air, and the rest strongly inflammable. The cabbage was putrid and highly offensive. That in the sun had yielded an ounce measure and a half of air, a very small proportion of which, perhaps one twentieth, was fixed air, and the rest slightly inflammable, the cabbage offensive.

This experiment shews that without light inflammable air is produced by the putrefaction of vegetable substances, and accounts for the production of this kind of air in marshes. The reason why the cabbage in the sun also produced inflammable air (though it was not in so great a quantity as from the cabbage in the dark) was that the mass of it was too great for the capacity of the vessel. There had also been very little sunshine, the weather having been rainy, or cloudy.

On the 28th of June I put fourteen pennyweights of *lettuce* into a jar containing twenty ounce measures of rain water. On the third of July it became turbid,

turbid, and two ounce measures of air were produced, the slightest proportion of which was fixed air, and the rest strongly inflammable. The lettuce had a very offensive smell. In this case, as in the former, the quantity of lettuce, as I imagined, was too great for the production of pure air.

A branch of garden *spurge* put into a jar of rain water, the 28th of June, had yielded but a few bubbles of air on the 17th of July, neither fixed air nor inflammable, but of the standard of common air. I then replaced the spurge in a quantity of fresh water, and on the 27th of July I took from it an ounce measure and an half of air, so pure that, with two equal quantities of nitrous air, the measures of the test were 0.66; and it would probably have yielded more air. At the time of the first observation I imagined the plant was not sufficiently putrid.

The green vegetable matter upon this plant was of a peculiar species, quite different from any thing that I had ever observed before, or have seen since. One of the berries of the spurge was quite covered with it, and exhibited the appearance of such a figure as is generally drawn to represent the atmosphere of a comet. It consisted of filaments as fine as a hair, each of them about half an inch in length, rising perpendicularly from the surface of the berry. This was probably the proper *conserua fontinalis*.

The next experiment exhibits very clearly the difference between the effect of *light*, and *no light*, with respect to the object of this inquiry. On the 30th of July I placed half a *cucumber*, weighing fifteen pennyweights, in a vessel containing seventy ounce measures of water, in the sun; and on the 24th of August I took from it one ounce measure of air, so pure that, with two equal quantities of nitrous air, the measures of the test were 1.0, not in the least inflammable, and without any mixture of fixed air. The cucumber was quite covered with the green vegetable matter, and had no bad smell.

At the same time the other half of the same cucumber, which had been kept in a vessel of the same size in the *dark*, had yielded one third of an ounce measure of air, all of which was phlogisticated, and the cucumber was very offensive. In this case I doubt not that the air in its nascent state, as it may be called, was inflammable air, but had been changed into phlogisticated air, as inflammable air is apt to be; in which case the quantity is always greatly diminished. Of this I shall produce several instances in the course of this volume.

The only *flowers* I made trial of were *white lillies*. Of these, on the 28th of June, I put three pennyweights into a jar containing about forty ounce measures of rain water; and at one time during the pro-

cess they seemed to have yielded about an ounce measure of air; but on the 17th of July the quantity was manifestly diminished, and when examined it appeared to be without any mixture of fixed air, and very nearly phlogisticated, the measures of the test being 1.7. The lillies had no bad smell. I doubt not but the phlogiston, which is always exhaled in great quantities from flowers, had contributed to diminish, and phlogisticate, the better air that had been first produced, though there had been but little or no appearance of green matter in this vessel.

Potatoes I found to afford an excellent pabulum for this vegetable matter, and consequently to be exceedingly favourable to the production of pure air, but seemingly not at all when they are boiled.

On the 24th of July, a potatoe, weighing two ounces, two pennyweights, twelve grains, cut into thin slices, was put into a jar containing a hundred and fifteen ounces of rain water, and placed in the sun. In a day or two the water became turbid, and air began to be emitted, the potatoe being quite covered with the green matter; and on the 28th all the water in the vessel was so full of green matter floating in it, that nothing could be seen in the inside of it. At the same time a low jar, containing about six ounces of water, with a small potatoe, not sliced, had nearly the same appearance.

Afterwards,

Afterwards, on the 3d of July, I put some slices of potatoe into a tall jar containing six ounce measures of water fresh distilled, having a communication with the water in the bason in which it was inverted by a glass tube, with a very fine orifice in the cork with which the jar was closed. About the 20th of August I observed these slices of potatoe to be a little green, and on the 24th they were wholly so, the green matter first appearing in the bason in which the jar stood, which was supplied from time to time with rain water.

In order to try what quantity of air I could procure by means of these potatoes, which appeared to be so well adapted to the purpose, I put three of them, each about the bigness of a small walnut, into a vessel containing thirty five ounces of rain water. They yielded five ounce measures of air, so pure that, with two equal quantities of nitrous air, the measures of the test were 0.54. The potatoes were quite soft, but could not be said to be offensive. Again, from a sliced potatoe weighing two ounces, two pennyweights, twelve grains, exposed to the sun from the 24th of July, in a jar containing 115 ounces of rain water, I took, on the 6th of August, ten ounce measures of air, the measures of the test, with two equal quantities of nitrous air, being 0.58, the potatoes quite soft as those above.

Lastly,

Lastly, from fifteen pennyweights of *boiled potatoes*, which had been exposed in the sun a long time, in a small receiver, I took about half an ounce measure of air, a small proportion of which was fixed air, and the rest phlogisticated air. This potatoe was never green. What would have been the result if the quantity of water had been greater, I cannot tell.

From three slices of *turnip*, exposed to the sun in a vessel containing ninety ounces of water, I took nine ounce measures of air, so pure that, with two equal quantities of nitrous air, the measures of the test were 0.75.

Nothing I ever tried was, in general, more unfavourable to the production of pure air than *onions*. It was only by using a very small quantity of them, and by exposing them to the sun in a very large quantity of water, that I succeeded to make it admit the green matter. At length, however, from five pennyweights and a half of onion, exposed to the sun in a jar containing 200 ounces of water, from the 6th of August to the 31st, I got six ounce measures of air, not in the least inflammable, and so pure that, with two equal quantities of nitrous air, the measures of the test were 1.2. At the same time I had exposed thirteen pennyweights, twenty three grains of the same onions in a jar containing three ounces and a half of water, and on the 9th of

October following I took of it a little more than half an ounce measure of air, all of which was phlogisticated. It extinguished a candle, and was not at all affected by nitrous air. There had been twice as much air in the vessel a month or six weeks before, and then it was probably inflammable.

SECTION X.

Of the Production of Air by Means of the Green Matter from Animal Substances.

ANIMAL substances were not, upon the whole, more favourable to the growth of this green vegetable matter, and the production of pure air from it, than vegetables; and different kinds of animal substances exhibited as great differences in this respect.

One of the first and most remarkable appearances that I had of this kind occurred in some experiments that I was making with *fishes*. It shews how readily the seeds of this aquatic vegetable find their proper pabulum,

pabulum, notwithstanding a great mass of water be in their way to it.

On the 13th of June I put three very small fishes into a jar containing 200 ounces of rain water, inverted in a basin of the same, when there was presently a thin filmy substance peeled off from all the surface of the fishes. After this a red matter, I suppose dissolved blood, issued from them, and was diffused through the whole mass of water, making it very turbid. About the 23d of June the red matter became, as it were, green, the green vegetable substance adhering to it; and on the 26th the whole mass of water was exceedingly green, and quite opaque; but the densest part of the green matter adhered to the fishes themselves, which always swam on the top of the jar. I did not examine this air till the 15th of July, when I found four ounce measures of it, and tolerably pure, but not so much so, I am persuaded, as I should have found it some time before. With two equal quantities of nitrous air, the measures of the test were 1.24.

A quantity of *beef* exposed to the sun in a vessel of water soon became green, and yielded air; but presently the green matter, which had been diffused through the whole mass of water, became yellow, or white; and from that time no more air was produced. The flesh was putrid and offensive. The

green vegetable, I doubt not, was quite dead, through the extreme putridity of the flesh, and the foulness of the water, which it had not been able to purify.

To try the difference between the effects of *light* and *darkness* with an animal substance, as I had done before with vegetables, on the 17th of July, I put eight pennyweights, ten grains, of roasted beef into a vessel containing about thirty ounces of water, and placed it in the sun, and an equal receiver, with an equal quantity of the same beef in a dark room. On the twentieth I perceived no appearance that struck me, but on the 21st in the evening, I found the flesh in the sun quite green, and two or three ounce measures of air were generated; but the water in the dark room continued quite transparent, and in every respect that I could perceive, unchanged.

On the 26th the green colour of the flesh and of the water in the sun began to disappear, and the vessel had a cloudy appearance. Soon after I examined the air, and found eight ounce measures, very pure, the flesh soft and putrid, but still green on its upper surface. The jar, which had been placed in the dark never had any air, nor was any produced from it afterwards, when it was removed into the sun.

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On the 17th of August I exposed in the sun, in a large retort of rain water, three pennyweights, six grains of roasted beef, the neck of the retort being plunged in water nine inches deep in a jar that nearly fitted it, and moreover closed with a cork, in which was a very small perforation, so as to give it as little communication with the external air as possible.

On the 9th of September I took from it two thirds of an ounce measure of air, all inflammable. The flesh had never turned green. During the same time I had exposed eight pennyweights, six grains, of the same beef in a jar containing two hundred ounces of pump water, which had turned green, and yielded dephlogisticated air. In the former case the beef was more in proportion to the quantity of water, and had also a very obstructed communication with the external air, from which alone the seeds of this green vegetable could come.

This process with a small quantity of *veal* was very remarkable, as this substance continued to be green, and give air, till every thing in it that could be offensive was quite exhausted.

On the 28th of June I put fourteen pennyweights of boiled veal into a large jar of rain water, and on the third of July both the upper part of the veal, and all the water, was quite green. On the fourth of July I took out half of the veal, and examining

the air, I found it to be nine ounce measures, no part fixed air, and so pure that, with two equal quantities of nitrous air, the measures of the test were 0.82. The water was still very green.

Part of this veal, which was then quite soft, I replaced in a jar of fresh water, putting the remainder of it into a small jar. This never gave any air at all. But on the 18th day of July, the water in the large jar was all very green, and in two days yielded five or six ounce measures of air. A little time after I examined it, and found twelve ounce measures, so pure that, with two equal quantities of nitrous air, the measures of the test were 0.57. The flesh had no coherence, and still was offensive; but on the 29th of July I took from it four ounces of air equally pure with the former, and on the sixteenth of August half an ounce more, and then the jar had nothing offensive in it.

The process with a roasted *tendon* of a calf's neck went on just as the above, with this difference, which I thought to be a little remarkable, that all the water was of a reddish hue before it became green, though there was no blood, or any thing red, in or about the tendon. The air which it yielded afterwards was very pure.

Perhaps the most satisfactory experiments that can be made with respect to the production of pure air, by means of this green vegetable substance, the
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pabulum that putrefaction affords it, the effect of light upon it, and again the influence of putrefaction to destroy that air, were some that I made with a *mouse*, which I always found most effectual for any purpose in which putrefaction was required, far more so than pieces of solid meat of any kind.

On the 21st of June I put a dead mouse into a jar containing two hundred ounces of water, inverted in a basin of the same, which I placed in the sun. At the same time I put another mouse into a jar of the same size, filled with the same water, and placed it in the dark. In this vessel the water was never discoloured, and very little air was produced; whereas from the mouse in the sun, there presently issued a quantity of white mucous substance, which soon turned to an intense green, and yielded air most copiously. After some time the whole jar was full of this thick green matter, and air rose from every part of it; but it was destroyed as soon as it approached the upper part of the jar, where the dead mouse floated, owing no doubt to the phlogistic matter which issued from it.

In order to verify this, I threw out the mouse, and dividing the turbid green water into two parts, I put one half of it into a retort exposed to the sun, and the other into an equal retort which I placed in the dark. The water in the sun presently yielded

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permanent air, highly dephlogisticated; whereas that in the dark gave not a single bubble, but when I soon afterwards brought it into the sun, it yielded air like the other.

The preceding experiments being made chiefly with the *muscular parts* of animals, I had the curiosity to try what difference would be made with the other parts of the system, and some of the secretions; but I was contented with a few articles under this class, as the extension of the experiments to all parts of the animal system would have been tedious, and did not seem to promise much advantage.

By means of a quantity of the *brain* of a sheep, and also of the *lungs*, and of the *liver*, I procured a very considerable quantity of very pure air, the process with each of these being exactly like those which have been already described, and therefore not requiring to be repeated. These substances immersed in rain water were presently covered with the green vegetable matter, which was also diffused through the whole body of the water, and the produce of air from it was very copious.

The experiments I made with *blood*, *fat*, *gall*, and *gravy* had different results.

Eighteen pennyweights of the crassamentum of sheep's *blood*, was exposed to the sun in a jar of rain water, containing two hundred ounces; but it was
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always red, and never yielded more than an ounce measure of air, the whole of which was phlogisticated.

No air at all was produced from a small piece of *fat mutton*, exposed in the same manner ten days, nor from water which had a small quantity of *mutton gravy* in it.

About half an ounce of *sheep's gall* was exposed, together with the gall-bladder in which it was contained, on the 25th of July, in a vessel containing two hundred ounces of water, which in a few days was green, and produced air; but before the 16th of August it was almost all absorbed, and some time after was wholly so. Gall, being a very putrescent substance, might act as the mouse in the experiment recited above; so that perhaps with a less quantity of gall, or by withdrawing it in time, I might have succeeded better.

It is impossible not to observe from these experiments, the admirable provision there is in nature, to prevent, or lessen, the fatal effects of putrefaction, especially in hot countries, where the rays of the sun are the most direct, and the heat the most intense. For whereas animal and vegetable substances, by simply putrefying, would necessarily taint great masses of air, and render it wholly unfit for respiration, the same substances putrefying in water,

water, supply a most abundant pabulum for this wonderful vegetable substance, the seeds of which appear to be in all places dispersed invisibly through the atmosphere, and capable, at all seasons of the year, of taking root, and immediately propagating themselves to the greatest extent. By this means, instead of the air being corrupted, a vast addition of the purest air is continually thrown into it.

By this means also stagnated waters are rendered much less offensive and unwholesome than they would otherwise be. That froth which we also see on the surface of such waters, and which is apt to create disgust, generally consists of the purest dephlogisticated air, supplied by aquatic plants which always grow in the greatest abundance, and flourish most, in water that abounds with putrid matter. When the sun shines these plants may also be seen to emit great quantities of pure air.

Even where animal and vegetable substances putrefy in *air*, as they have some moisture in them, various other plants, in the form of *mold*, &c. find a proper nutriment in them, and by converting a considerable part of the phlogistic effluvium into their own nutriment, arrest it in its progress to corrupt the surrounding atmosphere. So wonderfully is every part of the system of nature formed, that good never fails to arise out of all the evils to which,
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in consequence of general laws, most beneficial to the whole, it is necessarily subject. It is hardly possible for a person of a speculative turn not to perceive, and admire, this most wonderful and excellent provision.

SECTION XI.

Of the Property of the Willow Plant to absorb Air.

OF the various plants on which I made experiments in the course of the summer of 1777, I met with one which had the remarkable quality of absorbing a great proportion of any kind of air to which I exposed it. It is the *epilobium hirsutum* of Linnæus, in English the *willow plant*, and it grows best in the water of marshy ground. The method in which I made the experiments was by fixing the jar of air with its mouth in the water in which the plant grew, keeping it upright, by fastening it to a stick fixed in the bottom of the pool, then bending the plant under the water, and introducing the top of it into the inside of the jar.

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I presently found that the common air to which it was exposed in this manner was considerably diminished, and rendered noxious; but having neglected one of these jars for about a week, I was surprized to find that near one half of the whole quantity of air was absorbed, the water having risen so far within the jar: whereas, in general, the diminution of air occasioned by what I suppose to be mere phlogiston, as in the process of iron filings and sulphur, or the calcination of metals, &c. does not exceed one fourth of the whole. Supposing, however, that I might not have taken sufficient notice of the quantity of air originally contained in the jar, I repeated the experiment in a jar about ten inches long, and one in diameter, and found, after some time, that the diminution went unquestionably beyond one fourth of the whole; and then, to prosecute the experiment farther, I introduced other plants of this kind into jars about nine inches in length, and two inches and a quarter in diameter, one of them filled with inflammable, and the other with nitrous air.

After about a fortnight, I noted the state of these plants, and of the air to which they were exposed, and found them to be as follows. The plant which had been exposed to common air, in the jar about ten inches long and one inch wide, and which had been, in all, about a month in that situation, had absorbed

absorbed seven eighths of the air in the jar. The plant was quite yellow and dead; but though it had been so for some time, it had still continued to absorb the air.

The plant which had been confined only about a fortnight, in one of the larger jars of common air, was quite green, and had consumed three fourths of it.

The plant in a jar of the same size, containing inflammable air, had consumed one third of it, and part of the remainder (which I drew from it) was, to all appearance, as inflammable as ever it had been. The plant was green.

The plant in the nitrous air was yellow and dead, and had consumed one third of its air.

In this state I was obliged to leave these plants, and to suspend all my other experiments on plants by my journey to the sea side; but I had accounts sent me of the state of them from time to time, by which it appeared, that the air continued to diminish till the common air in the narrow jar was only one tenth of its original quantity, the inflammable air was reduced to one seventh of the whole; and the air in the other jars was diminished in about the same proportion. But at length, the summer being very dry, the water failed, and the common air, of course, got into the jars. I regret, particularly, that I had no opportunity of examining the
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state of the *inflammable* air in the last stage of its diminution.

Finding this plant to absorb so much air, I was desirous of knowing what became of it, whether it was incorporated in the substance of the plant, or was merely strained through it. For this purpose I put the root of one of the plants, with all the earth that adhered to it, into a jar; and bending the plant a little, placed the jar in such a manner, as that the mouth of it was just immersed in a pan of water, and the plant, though in an awkward situation, grew pretty well; the upper part being supported, and also turning upwards of itself, by its natural growth.

Some air was certainly strained through this plant; but much less than I had expected, considering the quantity that I supposed it would have absorbed in the same time, at least if it had grown freely in its natural situation. The air which I collected in this manner was almost intirely phlogisticated, as was always that which remained of the common air that the plant had absorbed.

To try whether the plant would actually absorb air in the situation above described, when the root was confined in a jar of water, I gave it another bend near the top, and placed a jar of common air over it, standing in another vessel of water; but the plant would not bear so much torture, and
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though it did not die immediately, it decayed gradually, and the experiment had no effect.

It will certainly be well worth while to compare all the circumstances in which air is *absorbed*, as well as those in which it is merely *diminished* to a certain degree, in order to ascertain the circumstances that are common to all the cases, and thereby discover the proper cause of this remarkable phenomenon. Water, and many other fluids, have this property in some degree, as has long been known to natural philosophers, who did not give much attention to the *quality*, or *chemical properties* of air. I discovered it in a still greater degree in *oil of turpentine*. And that excellent philosopher the Abbé Fontana has discovered it in a much greater degree still in *charcoal*. This plant, however, seems to possess the same property in as great a degree as charcoal. It only requires more time to produce its effect. At another opportunity I propose to examine this matter a little farther. At present, no conjecture occurs to me that I think worth communicating to the public.

SECTION XII.

Of the Growth of the Willow Plant in different Kinds of Air.

IN the last section I observed that the willow plant grew very well both in inflammable and in common air, and that it absorbed a considerable proportion of both the kinds, as well as of nitrous air. In this there could not possibly be any mistake, unless we suppose the water to have absorbed the air, which it has never been known to do in any similar circumstances. However, when I resumed the experiments on the growth of this plant in the course of the summer of 1778, I had no instance of the absorption of common air; but I had repeated, and very extraordinary ones of the absorption of inflammable air by it, and the plant flourished so remarkably in this air, that it may be said to feed upon it with great avidity. This process terminates in the change of what remains of the inflammable air into phlogisticated air, and sometimes into a species of air as good as common air, or even better; so that it must be the *inflammable principle*

principle in the air that the plant takes, converting it, no doubt, into its proper nourishment.

Some other plants also, as *comfrey* and *duck weed*, I observed to thrive very well in inflammable air, and to produce a similar effect upon it, though, as I observed in my first publication on the subject of air, and upon other occasions since, *mint* does not thrive so well in this as in common air, and I have generally found that, in time, this plant is killed by it.

It may deserve to be mentioned in this connexion, that the willow plant grows best in marshy places, which abound with inflammable air. The plants that I made use of grew in the bottom of a field, in and near a piece of water, into which, if I only thrust a stick, a prodigious quantity of inflammable air rushed out, so that, without changing my place, I could, at any time, collect a large receiver full of it; and bubbles of air were very frequently rising spontaneously from the mud at the bottom. It may therefore be a provision in nature, that this noxious kind of air should be fitted to the nourishment of such plants as grow best in those places in which it abounds, as well as that plants in general should purify the common atmosphere.

The facts from which these conclusions are drawn, as well as some farther observations on the subject, are the following, in the recital of which it will be

necessary to mention the month and the day on which the observations were made, as they have a connexion with the state of the plant, and probably with its power of action on air.

On the 26th of May, 1779, I put a jar of about twenty ounce measures of air over a willow plant growing in water, and on the first of June I observed that the air was little diminished in quantity, or affected in quality; for by the test of nitrous air the measures were 1.33; that is, when one measure of nitrous air was mixed with one measure of this air, they occupied the space of 1.33 measures. The plant continuing to grow, I examined it on the 5th of June, when the measures were 1.3, and those of the common air at the same time, I observed, were 1.26. This slight degree of injury I imputed to some black leaves, which were then about the plant. On the 8th of the same month, the measures were 1.36, and on the 15th they were 1.4, and there was still no more prospect of the air being absorbed than before. This I thought very extraordinary, as in the preceding summer I had always found, take what care I could, that these plants injured common air, and at least diminished it in the usual degree of one fourth, if they did not absorb more of it.

In inflammable air, the results were consistent with the preceding observations, and uniform with them-

themselves. But the year before I had no opportunity of pursuing these observations to the extent that I wished, so that I could not tell in what state the plants would finally leave the air; whereas now I had sufficient time fully to satisfy my curiosity in this respect.

On the 18th of May I introduced one of these plants, growing in water, under a jar of strong inflammable air, and the 3d of June following, I found that it was diminished about one third. Examining it, I found it was but weakly inflammable. This plant had not room to expand itself, but still it lived very well. On the 31st of the same month, there was no more than one third of the air remaining in the jar, and it was still slightly inflammable. Owing to some accident or other, the plant had been dead about a week, after which time I observed that the air had ceased to be diminished.

I then introduced another plant into what remained of the air, and on the 5th of June it was reduced one third more, and then I could not perceive that there was any thing inflammable in it. It was also a good deal dephlogisticated; for with two equal quantities of nitrous air, the measures of the test were 1.6, so that, upon the whole, the growth of this plant in this kind of air had the same effect upon it as agitation in water would have had, viz.

diminishing it, depriving it of its inflammability, and rendering it in some measure respirable.

I had another result exactly corresponding with this. For on the 9th of June I examined a quantity of inflammable air, in which a willow plant had grown from the 26th of May; but in this case not more than about half the quantity was absorbed, but part of the remainder fired with one explosion, like a mixture of common and inflammable air; and applying the test of nitrous air, the measures were 1.43, which is about that state of air in which a candle just goes out. On the 15th of June another quantity of this air, in which a willow plant had grown from the same time, was fired in the same manner, and the measures of the test were 1.44; though only about one half of it had disappeared.

Another quantity of the same kind of inflammable air, in which a willow plant had grown the same time, was reduced to one sixth of its original quantity. It then exploded like a mixture of common and inflammable air, and the measures of the test were 1.53. It was examined on the 9th of June. On the 15th of the same month it was diminished still more, and had then nothing inflammable in it, but the purity was nearly the same; the measures of the test being 1.54.

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In all the experiments that I made of this kind, the quantity of air absorbed was very various, depending probably, upon the health of the plant, its size in proportion to that of the jar, and other circumstances.

On the 24th of May I had introduced one of these plants into a jar of inflammable air, collected from the marsh near which I had gathered it; and on the 9th of June I had found it so far diminished, that little more than one seventh of the original quantity remained. This was merely phlogisticated air; for it was not affected by nitrous air, and extinguished a candle.

On the 15th of June, I found that another quantity of the same kind of inflammable air, in which a willow plant had grown from the same date, was not diminished near so much; for about one third of the original quantity was left. This, however, was partly inflammable, the slightest blue flame imaginable being perceived in a large jar of it. When I applied the test of nitrous air, the measures were 1.62.

A sign of the great vigour of the plants growing in inflammable air, was the vivid greenness not only of the leaves that were in the air, but of those also that were under water, and the length of time that they continued so in these circumstances; whereas, in general, when the top of the plant was

in common air, the leaves that were under water soon became discoloured, and perished. These leaves on the contrary, not only continued green, but were always loaded with air bubbles, which were continually detaching themselves, and rising into the jar, having their places supplied by others. These bubbles, I had no doubt, consisted of the air that had been strained, as it were, through the plant, leaving its phlogiston behind, for the nourishment of the plant. I endeavoured to collect a quantity of these bubbles, before they mixed with, and diluted, the inflammable air in the top of the jar, but I did not succeed. I have no doubt but that it would have been dephlogisticated air, as this will easily account for the state in which I found this air in the experiments recited above.

It was doubtful, however, whether these bubbles consisted of air that had been imbibed by the leaves, and then passed through a considerable space within the substance of the plant, or of the air that had been contained in the water, to which these leaves had immediate access. The latter seems more probable from some experiments, but the following are nearly decisive in favour of the other supposition.

I put the stalk of a willow plant into an inverted jar full of water, while the top of it was in a jar of inflammable air. In these circumstances a
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small quantity of air was collected in the inverted jar, and it was evidently better than common air. This air I had observed to come from all the outside of the stalk, but especially from the places where the leaves had been broken off; and there were some few bubbles from the middle of the place where the stalk itself had been cut, for it had no root.

In another experiment of this kind, when the plant had been in the situation above described, from the 11th to the 14th of June, three fourths of an ounce measure of air was collected in the inverted jar, so pure, that the measures of the test were 0.63; and with two measures of nitrous air 1.5. Applying the flame of a candle at the orifice of a tube filled with it, there was a loud explosion, so that it was a mixture of dephlogisticated and inflammable air.

On the 19th of June, I collected half an ounce measure more from the same plant; and applying to it the test of nitrous air, the measures were 0.9, and there was nothing sensibly inflammable in it. Had there been nothing inflammable in the air collected in the inverted jar, containing the stalk of the plant, the probability would have been, that all the air came from the water, dephlogisticated by the action of the plant; but the mixture of inflammable air in it seems to prove that part of

it, at least, had been imbibed by, and strained through the plant, entering at the leaves (which alone were exposed to the inflammable air) and issuing at the stalk, which was turned up into the other jar in which the air was received. This singular case, for it is the only result I ever had of the kind, shows that the plant had taken in more nourishment than it could properly digest.

This plant thriving so remarkably well in inflammable air, and depriving it of its inflammability, I thought it could not well fail to purify phlogisticated air, if I gave proper attention to its health and ease in its confined situation, though (perhaps through want of this attention) it had failed to do so the preceding summer; and I was not disappointed in my expectations at this time.

On the 22d of June I introduced one of these plants into a jar of air phlogisticated by the putrefaction of fishes, confined by rain water, in which I had found by frequent trials, that the green vegetable matter was not soon generated; and on the 26th of the same month, it was so much improved, that the measures of the test were 1.38, which is a little better than the state in which air will just extinguish a candle. The 3d of July, I examined it again, and then the measures were 1.32, and on the 15th of the same month, it was exactly of the standard of common air. The water by which it
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was confined certainly produced no air: for another jar filled with water, in the same trough, and therefore having precisely the same exposure with respect to light, and all other circumstances, had no air at all in it. A very little air was strained through this plant, and it was almost thoroughly phlogisticated; for the measures of the test were 1.7.

Nitrous air I have always found to be fatal to vegetable, as well as to animal life, and so it proved in this instance; as indeed it had done the preceding summer. From the 18th of May to the 18th of June, a quantity of this air was diminished by a willow plant to one fourth, and then it was so changed, that it admitted a candle to burn in it with a gently blue enlarged flame; a state which, as I have observed, nitrous air generally passes through before it becomes mere phlogisticated air, and which appears to be nitrous air partially dephlogisticated.

Phlogiston being the pabulum of plants, as it is probably of animals too, dephlogisticated air must (as indeed I had found before) be unfavourable to the growth of plants in general; and I constantly found it to be so in the case of the willow plant. To give it the fairer trial, I introduced a small, but healthy plant, growing in the marsh, into a jar of this air, so large that the plant was not in the least incommoded, and it did not reach the top of the

jar by several inches. This was done the 18th of May, but it died presently, and before the air was sensibly diminished; which was the case afterwards, owing, probably, to the putrefaction of the plant. But even then, being examined with two equal quantities of nitrous air, the measures of the test were 1.0.

Having filled a large earthen pot with water, and having sticks thrust into the earth quite round it, for the convenience of fastening jars with their mouths inverted in water, in order to fill them with different kinds of air, and introduce plants into them, without the trouble of going to the marsh in which they grew, as in Pl. VI. fig. 1; I filled one of these jars with dephlogisticated air, and then introduced the top of a willow plant into it. In a day or two, all the part that was within the jar began to turn white, and was soon after manifestly quite dead, even when many shoots of the same plant that were under water continued green, and looked well a considerable time afterwards. The air, being examined, was found to be very little injured. I therefore think we may safely conclude, that dephlogisticated air is universally hurtful to plants; and this, *a priori*, would be an argument in favour of the depuration of atmospherical air by vegetation.

Having made the preceding experiments on inflammable air with the willow plant, I proceeded
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to try a few other plants; and without giving such particular attention to these as those of the willow plant, I soon found that *comfrey*, which is hairy like the willow plant, and grows best in the same situation, and also the *meadow sweet* grew very well in inflammable air. So also did *duck-weed*, which was always remarkably healthy, and of a deep green colour, a certain sign, I believe, of health and vigour in plants in general; whereas, in dephlogistigated air, *duck-weed* always presently became pale, and died.

PART

P A R T II.

EXPERIMENTS AND OBSERVATIONS RELATING TO
RESPIRATION.

SECTION I.

Observations on Respiration, and the Use of the Blood.*

THERE is, perhaps, no subject in physiology, and very few in philosophy in general, that has engaged more attention than that of the use of *respiration*. It is evident, that without breathing most animals would presently die; and it is also

* This Section was a paper presented to the Royal Society, read Jan. 25, 1776, and is printed in the Philosophical Transactions, Vol. LXVI. p. 226. It will be perceived that when I made the experiments recited in this section, I supposed the phlogistication, as I called it, of air, to be the effect of phlogiston, emitting by the phlogisticating substance, and that I had no idea of the absorption of dephlogisticated air, which was the discovery of Mr. Lavoisier.

well known, that the same air will not long answer the purpose: for if it has been frequently respired, the breathing of it is as fatal as the total deprivation of air. But by what property it is, that air contributes to the support of animal life; and why air that has been much breathed will no more answer the purpose, seems not to have been discovered by any of the many philosophers and physicians who have professedly written upon the subject; and it might have continued to elude all *direct investigation*, when it discovered itself, without any trouble or thought, in the course of my researches into the properties of different kinds of air, which had at first quite another object.

In these experiments it clearly appeared, that respiration is a *phlogistic process*, affecting air in the very same manner as every other phlogistic process (*viz.* putrefaction, the effervescence of iron filings and sulphur, or the calcination of metals, &c.) affects it, diminishing the quantity of it in a certain proportion, lessening its specific gravity, and rendering it unfit for respiration or inflammation, but leaving it in a state capable of being restored to a tolerable degree of purity by agitation in water, &c. Having discovered this, I concluded that the use of the lungs is to carry off a putrid *effluviu*, or to discharge that phlogiston, which had been taken into
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the system with the aliment, and was become, as it were, *effete*; the air that is respired serving as a *menstruum* for that purpose.

What I then concluded to be the use of *respiration* in general, I have now, I think, proved to be effected by means of the *blood*, in consequence of its coming so nearly into contact with the air in the lungs; the blood appearing to be a fluid wonderfully formed to imbibe, and part with, that principle which the chemists call phlogiston, and changing its colour in consequence of being charged with it, or being freed from it; and affecting air in the very same manner, both out of the body and in the lungs; and even notwithstanding the interposition of various substances, which prevent its coming into immediate contact with the air.

As it may not be unpleasing or unuseful, I shall, before I relate my own experiments, briefly recite the principal of the opinions which have been held concerning the use of respiration, from Haller's excellent *system of physiology*, and some other of the most eminent writers upon that subject.

Hippocrates reckoned air among the *aliments* of the body. But it was more generally the opinion of the ancients, that, there being a kind of *vital fire* kept up in the heart, the heat of the blood was tempered in the lungs. Galen also supposed, that there was something equivalent to a fire constantly kept

up in the heart; and that the chief use of the lungs was to carry off such vapours as were equivalent to smoke thrown off from that fire. Haller, Vol. III. p. 354. Descartes maintained the same vital fire in the heart, supposing that air was necessary for cooling and condensing the blood. *Ibid.* p. 321.

Of the more modern physiologists, some have thought that the air itself is taken into the blood; others, that it is only something extracted from the air, as the more subtle parts of that fluid, an ether, or aerial nitre; while others suppose it to be the air itself, but dissolved in water, and therefore in an unelastic state. *Ibid.* p. 321.

Most of those who think that air is taken into the blood, suppose it to be taken in by the lungs, *ibid.* p. 330. Some suppose, that the effect of the admission of this air into the blood is a fermentation, p. 332. Others suppose, that it acts by its spring, preventing the too close contact of the globules, and thereby preserving its fluidity, intestine motion, and heat, *ibid.* Bertier supposed, that the circulation of the blood was, in a great measure, owing to the admission of air into it. Van Helmont ascribed the volatility of the fixed elements in the food to this air, p. 336; and Stevenson thought, that the air which had circulated in the blood, and which had heated the blood too much, was exhaled by the lungs, p. 355.

Others

Others say, that the air itself is not admitted into the blood, but only some active, spirituous, and ethereal particles; that this vital spirit passes from the lungs to the heart and arteries, and at length becomes the animal spirits, which are by this means generated from the air, p. 333. Others, who do not admit that the animal spirits are derived from the air, still say that some other *vital principle* comes from thence. This vital principle Malpighius supposes to be a saline vapour; Lister, a hot, inflammable, sulphureous spirit; Vieussenius, a volatile acid salt, which keeps up the fermentation of the blood; and Bryan Robinson, the aerial acid, which preserves the blood from putrefaction; preserves also its density, and strengthens the animal fibres. For this reason he supposes it is that we feel ourselves refreshed in cold air, as it abounds with a more plentiful acid quality, p. 334. They who suppose that nitre is taken from the air into the blood, ascribe to that principle its fermentation, its heat, and its density, p. 334.

It is a received opinion, that one use of the lungs is to attenuate the blood, p. 359; and Malpighius adds, that by this means, the different particles of the blood become thoroughly mixed together; while others think that the blood is condensed in the lungs; and others, that the globules, and all the finer humours, receive their configuration there,

ibid.

ibid. Some, without considering the air as of any other use than to put the lungs in motion, think, that heat is produced in the lungs by the attrition of the blood in passing through them, *Misc. Taurin.* Vol. V. p. 36. The red colour of the blood has been thought by some to be caused by this attrition in the lungs; but Lower refuted this notion, chiefly by observing, that the attrition of the blood is greater in the muscles, from which, however, it always returns black, *ibid.* Vol. I. p. 74.

Dr. Whytt thought there was something of a vital and stimulating nature derived from the air into the blood, by means of which it made the heart to contract, Haller, Vol. III. p. 336.

Boerhaave says, that air not changed is deadly; not on account of heat, rarefaction, or density, but for some other *occult cause*, *Misc. Taurin.* Vol. V. p. 30.

Dr. Hales, who has thrown much more light upon the doctrine of air than all his predecessors, was equally ignorant of the use of it in respiration, and at different times seems to have adopted different opinions concerning it.

In his *Statical Essays*, Vol. II. p. 321, he supposes, that air is rendered alkaline by breathing, and corrected, in some measure, by the fumes of vinegar.

In agreement, as he observes, with Boerhaave, he says, p. 100, that the blood acquires its warmth chiefly in the lungs, where it moves with much greater rapidity than in any other capillary vessels of the body, Vol. II. p. 87; but that one use of the air is to cool the blood, p. 94; and he makes an estimate of the degree of this refrigeration. The red colour of the globules of blood, he says, p. 88, intimates their abounding with sulphur, which makes them more susceptible and retentive of heat than those bodies which have less of it.

He also supposes, p. 102, that another great use of the lungs is to attenuate and separate the globules of blood; and that the floridness of the arterial blood above the venal may, in a good measure, be owing to the strong agitation, friction, and comminution, which it undergoes in passing through them. In like manner, in an experiment which he made for the purpose, blood much agitated in a close glass vessel was observed to be very florid, not only on its surface, but through its whole substance, as arterial blood is, Vol. II. p. 102. I would observe, however, that in this experiment, the blood must have acquired its florid colour from the air with which it was agitated.

He adds, that it is probable, that the blood may, in the lungs, receive some other important influence from

from the air; which is in such great quantities inspired into them. In other places, however, he explodes the doctrine of a *vivifying spirit* in the air. It has long, he says, been the subject of inquiry to many, to find of what use it is in respiration; which, though it may in some respects be known, yet it must be confessed, that we are still much in the dark about it, Vol. II. p. 102.

Suffocation, he says, Vol. II. p. 271, consists chiefly in the falling flat of the lungs, occasioned by the grossness of the particles of a thick noxious air, they being, in that floating state, most easily attracted by each other, as we find that sulphur, and the elastic repelling particles of air are; and consequently unelastic, sulphureous, saline, and other floating particles, will most easily coalesce, whereby they are rendered too gross to enter the minute vesicles, which are also much contracted, as well by the loss of the elasticity of the confined air, as by the contraction occasioned by the stimulating acid sulphureous vapours. And hence it is not improbable, that one great design of nature in the structure of this important and wonderful *viscus*, was to frame the vesicles so very minute, thereby effectually to hinder the ingress of gross, feculent particles, which might be injurious to the animal economy.

Lastly, he concludes, that the effect of respiration is to abate, and in part destroy, the elasticity of the
air;

air; and as this was effected by sulphureous vapours, and he could breathe for a longer time air that had passed through cloths dipped in a solution of salt of tartar, he concluded, that the air had been mended by the tartar having strongly imbibed the sulphureous, acid, and watery vapours, Vol. I. p. 267.

Haller, after reciting the opinions of all that had gone before him, supposes, with Dr. Hales, that, in consequence of the air losing its spring in the lungs, they cannot be kept dilated; and therefore, they must collapse, and the circulation of the blood be impeded, Vol. III. p. 258. When he states his opinion concerning the use of the lungs more fully, he says, that the true use of them is partly inhaling, and partly exhaling, p. 351. That the lungs *inhale* both water and air; but that in the lungs the air loses its elastic property, so as to be easily soluble in water or vapour, p. 352, and he thinks it probable, that this air serves as a cement to bind the earthy parts together. He also makes no doubt, but that various other matters, miscible with water, are inhaled by the lungs; and he even thinks it not improbable, that the air may carry some electric virtue along with it. The principal *exhalation* of the lungs, he thinks, to be water, abounding with oily, volatile, and saline principles; and these oily and fetid vapours, he thinks, to be the *fuligines* of Galen and other ancients, p. 354.

Mr.

Mr. Cigna of Turin has given much attention to this curious subject, as appears by two memoirs of his: one in the first volume of the *Miscellanea Taurinensia*, in which he very well accounts for the florid red colour of the blood; and the other, which is a much more elaborate Memoir, intitled, *De Respiratione*, in the fifth volume of the same work, just published, or about to be published, the copy of the article having been sent to me by the author.

He takes it for granted, that air which has once been breathed is unfit for farther respiration, on no other account than its being loaded with *noxious vapours*, which discover themselves by a fetid smell. *Misc. Taurin.* Vol. V. p. 30. And he takes it for granted, that the elasticity of air is diminished by respiration, though he does not consider that diminution of elasticity as the cause of its noxious quality. He therefore concludes, that air which has been breathed, suffocates by means of the irritation which it occasions to the lungs, by which the bronchia, and the lungs themselves, are contracted, so as to resist the entrance of the air; and therefore, that respired air is noxious on the same account as mephitic vapours, or those of burning sulphur, p. 31; that, in frequently breathing the same air, it becomes so loaded with these vapours, as to excite a convulsion in the lungs, and thereby

render them unfit for transmitting the blood, p. 42.

This philosopher supposes that air enters the pores of the blood, retaining its elastic power, p. 50, and that it continues at rest there, because its endeavour to escape is counteracted by the equal pressure of the ambient medium, p. 52. This air, he supposes to be introduced into the blood by the chyle, and never by the way of the lungs, except when, by some means or other, the equilibrium between the air in the blood and the external air is lost, p. 57. If the external air be rarer than the internal, the air in the blood, expanding itself, will inflate the animal, and have the same effect as air introduced into the veins.

What we are chiefly indebted to M. Cigna for, is his decisive experiments with respect to the florid colour of the blood, which he clearly proves to be caused by the contact of air ; though he afterwards seems willing to desert that hypothesis. It was often imagined, that the reason why the lower part of a quantity of blood was black, while the surface was red, was, that the black particles, being heavier than the rest, subsided to the bottom ; but this opinion our author clearly refutes. He found, that when he put a little oil upon a quantity of blood, it remained black throughout ; but that when he took
away

away the red part, and exposed to the air the lower *laminae*, which were black, they also became successively red, till the whole mass acquired that colour. *Misc. Taurin.* Vol. I. p. 73. Also, at the request of M. Cigna, father Beccaria tried what would be the effect of exposing blood in *vacuo*; and he found, that in those circumstances, it always continued black; but that, by exposing it again to the air, it became red, p. 68.

M. Cigna concludes his first dissertation with observing, that it is not easy to say how it comes to pass, that the lower part of a mass of blood becomes black, whether by the air which it had imbibed escaping from it, or by its depositing something saline, necessary to contribute to its redness, or by the pressure of the atmosphere; but he inclines to think, that air mixed with blood, and interposed between the globules, preserves its redness: but that by concreting it is expelled from it, or becomes so fixed as to be incapable of making it red. This opinion, he thinks, is rendered in some measure probable, by the increased density of concremented blood, and by the emission of air from other fluids in a concrement state, p. 74.

Notwithstanding what he had advanced in his first Memoir, yet in the second, which was written several years after it, he doubts whether the change of colour in the blood takes place in the lungs: but

If it does, he inclines to ascribe this effect to the *evaporation* from the blood in the lungs: and though he always found, that the colour of the blood was changed by the contact of air, yet when he considered that evaporation must, as he thought, necessarily attend the contact of air, he imagined, that this effect might equally be attributed to this circumstance. But he acknowledges, that this hypothesis ought not to be received till it be confirmed by experiments, *Misc. Taurin. Vol. V. p. 61.*

Upon the whole, he concludes, that the principal use of air to the *blood*, is to preserve the equilibrium with the external air, and to prevent the vessels from being rendered unfit to transmit the blood, on account of the external pressure; whereas, by means of the air they contain, the fluids move in their proper vessels as freely as in *vacuo*, and the membranes and viscera also easily slide over each other, p. 63. And with respect to the use of the *lungs*, since he imagined that air is not introduced into the blood by means of them, he thinks, that because such lungs as those of man are given to the warmer animals only, the chief use of respiration is exhalation, and consequently the cooling of the blood, p. 65.

The last writer whom I shall quote upon this subject, is the late ingenious Mr. Hewson; who says, in his *Experimental Inquiry into the Properties of Blood*,

p. 9, "As the colour of the blood is changed by
" air out of the body, it is presumed, that the air
" in the lungs is the immediate cause of the same
" change in the body." That this change is really
produced in the lungs, he is persuaded, he says,
from experiments, in which he distinctly saw the
blood of a more florid red in the left auricle of the
heart, than it was in the right; but how this effect
is produced, he says, is not yet determined.

Since some of the neutral salts, and particularly
nitre, have a similar effect on the colour of the
blood; some, says he, attribute this difference to
the nitre absorbed from the air, while in the lungs.
But this, he adds, is a mere hypothesis; for air
contains no nitre, and most of the neutral salts pro-
duce the same effect in some degree.

After this review of the observations and opinions
of others on this important question in physiology,
I shall proceed to recite my own. It may appear
something extraordinary, that among such a variety
of opinions concerning the use of respiration, the
right one should never have been so much as con-
jectured, though unsupported by the proper proof.
But indeed, this animal function, and the phlogistic
processes in chemistry, especially that of the cal-
cination of metals, which is, perhaps, the most sim-
ple of them, are to appearance very different things;
and therefore, it is the less to be wondered, that no

person should have imagined, they would produce the same effect on the air in which they were performed.

That respiration, however, is, in reality, a true phlogistic process, cannot, I think, admit of a doubt, after its being found, that the air which has served for this purpose is left in precisely the same state as that which has been exposed to any other phlogistic process. And since all the blood in the body passes through the lungs, and, according to Mr. Hewson's observations and others, the remarkable change between the colour of the venal and arterial blood takes place there, it can hardly be doubted, that it is by means of the *blood* that the air becomes phlogisticated in passing through the lungs: and therefore, that one great use of the blood must be to discharge the phlogiston with which the animal system abounds, imbibing it in the course of its circulation, and imparting it to the air, with which it is nearly brought into contact, in the lungs; the air thus acting as the great menstruum for this purpose.

Though I had no doubt concerning this conclusion from my former experiments, I thought so great a problem deserved as much illustration as could be given to it; and therefore I was willing to try, whether blood was of such a nature, as to retain any of this power of affecting air when congealed,

congealed, and out of the body, that it has when it is fluid, and in the body ; and the experiments have fully answered my expectations.

Having taken the blood of a sheep, and let it stand till it was congluted, and the serum was separated from it (after which the surface, being exposed to the common air, is well known to assume a florid red colour, while the inside is of a much darker red, bordering upon black) I introduced pieces of the crassamentum, contained in nets of open gauze, or of wire, sometimes through water, and sometimes through quicksilver, into different kinds of air, and always found that the blackest parts assumed a florid red colour in common air, and more especially in dephlogisticated air, which is purer and more fit for respiration than common air (and accordingly the blood always acquired a more florid colour, and the change was produced in less time in this than in common air) whereas the brightest red blood became presently black in any kind of air that was unfit for respiration, as in fixed air, inflammable air, nitrous air, or phlogisticated air ; and after becoming black in the last of these kinds of air, it regained its red colour upon being again exposed to common air, or dephlogisticated air ; the same pieces becoming alternately black and red, by being transferred from phlogisticated to dephlogisticated air ; and *vice versa*.

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In these experiments the blood must have parted with its phlogiston to the common air, or dephlogisticated air, and have imbibed it, and have become saturated with it, when exposed to phlogisticated, nitrous, inflammable, or fixed air. The only difficulty is with respect to the fixed air; for all the other kinds certainly contain phlogiston. But as there are, perhaps, no examples of any substance losing one principle, without at the same time acquiring another; and in other experiments both inflammable and dephlogisticated air act upon each other through a bladder, the acidifying principle of dephlogisticated air may enter the blood through the membrane of the lungs, as well as phlogiston from the lungs unites with dephlogisticated air in them and so form fixed air*.

The blackness of the blood may arise from other causes than its acquiring phlogiston. Gaber, for instance, observes, that blood becomes black when it begins to putrefy, as it does also whenever it is dried and hardened near the fire. Father Beccaria also found, as I have observed, that red blood *continued* (and he could hardly fail to observe also, that it *became*) black in *vacuo*, where it could not have imbibed phlogiston. This I found to be the case when the blood was covered two inches and a half

* That this is actually the case, appears by the experiments of Dr. Goodwin, and some that I have made since the publication of this paper, and which will be recited hereafter. But I had not this idea so early.

with

with serum; but it regained its florid colour when it was exposed to the open air.

In general, however, it cannot be expected, that when blood has become black without having received phlogiston *ab extra*, it will recover its florid colour by being exposed to the air. For the delicacy of its texture, and consequently its capacity of being easily affected by phlogiston, may be essentially altered by internal causes of blackness. This is even the case when blood has become black by being exposed to nitrous and inflammable air, though this change is probably effected by its imbibing phlogiston.

I exposed pieces of the same mass of red blood to these two kinds of air, and also to fixed air at the same time. They all became black; but that which was in the inflammable air was the least so, and none of them recovered their florid colour in the open air. But at another time, a piece of crassamentum, which had become black in fixed air, did, in some measure, and very slowly, recover its florid colour in dephlogisticated air. Perhaps the pieces that had lost their colour in the nitrous and inflammable air might have recovered it by means of this more powerful menstruum.

Since, however, blood, after becoming black in phlogisticated air, is always capable of resuming its red colour on being again exposed to pure air, it may be concluded, that the preceding blackness, dis-

discharged in the pure air, and producing the constant effect of phlogiston, in depraving the air, was owing to the phlogiston it had imbibed in the former situation, and which it parted with in the latter. And this is remarkably the case when blood is transferred from phlogisticated into dephlogisticated air. Even the circumstance of the *deeper colour* is sufficient to give a chemist a suspicion that it contains more phlogiston than blood of a lighter colour.

When I had found how readily pieces of blood changed their colour, according to the quality of the air to which they were exposed, I proceeded to examine the state of that air, in order to observe what change had taken place in it; and as dephlogisticated air admits of a more sensible change of quality than common air, I gave it the preference in this experiment; putting a piece of crassamentum, about the bigness of a walnut, into the quantity of about five ounce measures of this air.

This process I continued for the space of twenty four hours, changing the blood about ten or twelve times; after which I found the air so far depraved, that whereas, at the beginning of the experiment, one measure of it and two of nitrous air occupied the space of no more than half a measure, the same mixtures afterwards occupied the space of a measure and a half. Now since air is universally depraved by phlogiston, and in this sense,

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I believe,

I believe, by nothing else, it is evident, that this black blood must have communicated phlogiston to the air; and consequently its change of colour from black to a florid red must have been occasioned by the separation of phlogiston from it.

The next day, when, of course, the blood was nearer to a state of putrefaction, in which every kind of substance, without exception, will injure respirable air, I put a quantity of red blood, tinged in a few places with black, which I could not easily separate from it, to about the same quantity of the same dephlogisticated air, and suffered it to stand, without changing, for the same space of time; when it was so little injured, that the measures above-mentioned occupied the space of only two thirds of a measure.

That blood has a power of taking phlogiston from air, as well as imparting phlogiston to air, I satisfied myself by exposing blood of a very beautiful florid colour to nitrous air, inflammable air, and phlogisticated air. The two first mentioned kinds of air were considerably diminished by the process, which was continued two days, during which time the blood had been changed five or six times.

The nitrous air, by this means, lost a great proportion of its power of diminishing, that is, phlogisticating, common air. For now two measures
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of common air and one of this occupied the space of two measures and a quarter, instead of one measure and three fourths. The inflammable air, though still inflammable, was rendered in some degree wholesome by the process; being, after this, considerably diminished by nitrous air, which is a state to which it is brought by agitation in water, and which, continued longer, deprives it of its inflammability likewise. It cannot be doubted, therefore, but that, in both these cases, the red blood, by becoming black, received phlogiston from these two kinds of air.

With respect to the phlogificated air, I only observed that, after a few hours exposure to the red blood, it was sensibly, but not much, diminished by nitrous air; which otherwise it would not have been in the least degree. This blood, however, was of the lightest colour; that is, according to my hypothesis, the most free from phlogiston, of any that I have ever seen; and I have tried the same thing, without success, with blood of a less florid colour, though as florid as the common air could make it. But it should be considered, that the proper function of the blood is not to receive phlogiston from *air* (not meeting with any phlogificated air in the course of its circulation) but to communicate phlogiston to air; and therefore, there is by no means the same reason to expect, that air will

will be mended by red blood, as that it will be injured by black blood.

It may be objected to this hypothesis, concerning the use of the blood, that it never comes into actual contact with the air in the lungs, but is separated from it, though as Dr. Hales states it, at the distance of no more than a thousandth part of an inch. The red globules also swim in a large quantity of serum, which is a fluid of a quite different nature.

In order to ascertain the effect of these circumstances, I took a large quantity of black blood, and put it into a bladder moistened with a little serum, and tying it very close, hung it in a free exposure to the air, though in a quiescent state; and the next day I found, upon examination, that all the lower surface of the blood, which had been separated from the common air by the intervention of the bladder (which is an animal membrane, similar to that which constitutes the vesicles of the lungs, and is at least as thick) and likewise a little serum, had acquired a coating of a florid red colour, and as thick, I believe, as it would have acquired, if it had been immediately exposed to the open air; so that this membrane had been no impediment to the action of the air upon the blood. In this case it is evident to observe, that the change of colour could not be owing to *evaporation*, as Mr. Cigna

conjectures. This experiment I repeated, without previously moistening the bladder, and with the very same result.

I observed also, that when I cut out a piece of the crassamentum, and left the remainder in the vessel with the serum, not only that part of the surface which was exposed to the air, but that which was surrounded with serum, and even covered with it to the depth of several inches, acquired the florid colour; so that this deep covering of serum, which must have effectually prevented all evaporation, was no more an impediment to the mutual action of the blood and the air, than the bladder had been. The serum of the blood, therefore, appears to be as wonderfully adapted to answer its purpose, of a vehicle for the red globules, as the red globules themselves: for the slightest covering of water, or *saliva*, effectually prevents the blood from acquiring its florid colour; and M. Cigna found that this was the case when it was covered with oil.

That it is really the *air*, acting through the serum, and not the serum itself, that gives the florid colour to the blood, is clearly ascertained by the following experiment. I took two equal portions of black blood, and put them into equal cups, containing equal quantities of serum, which covered them to the depth of half an inch. One of these cups standing in the open air, and the other being placed

placed under an exhausted receiver, the former presently acquired a florid colour, while the other continued twelve hours as black as at first. Being taken out of the receiver, it stood all night in the open air without becoming red, and continued black ever after, even when the serum was poured off.

I also more completely satisfied myself of the influence of the air upon the blood, through a body of serum, by the reverse of this experiment. For I found that red blood became black through the depth of two inches of serum, when the vessel containing it was exposed to phlogisticated air; so that the red globules of the blood both receive, and part with phlogiston by means of the air, notwithstanding the interposition of a large body of the fluid in which they naturally float.

It must not, however, be inferred in all cases, that blood becomes black by imbibing phlogiston *ab extra*. For if time be given for it, this change of colour may arise from *internal causes*, as from putrefaction, even when blood contains the least phlogiston possible. To try this, I took a small quantity of perfectly florid coloured blood, and putting it into a clean glass tube, sealed it hermetically; when I found that, notwithstanding it was by this means cut off from all communication with external substances, it became black in a few days.

And another quantity, kept in the same manner, but in a warm place, became black much sooner.

Except serum, *milk* is the only animal fluid that I have tried, through which the air can act upon blood: for black blood became red when it was plunged in milk, in the same manner as if it had been covered with serum. In urine, indeed, black blood becomes instantly red; but this is not owing to the action of the air, through the urine, but to the saline nature of that fluid. This makes it probable that the redness of the blood is owing not only to its parting with phlogiston, but to imbibing the acidifying principle from the dephlogisticated air.

In some cases, care must be taken to distinguish the floridness with which some detached parts of a quantity of blood are tinged, from that which penetrates the solid parts of it. In *saliva*, and in water impregnated with alkaline salt, fixed or volatile, and also in spirit of wine, the extreme angles and edges of pieces of crassamentum, and small detached parts, floating in those liquors, will appear of a very florid red, while the compact mass of blood continues dark. The florid colour of the prominent and detached parts, in these cases, seems to be the mere effect of the minute division of the parts of the crassamentum in the fluid in which those parts float;

when at the same time it has no such effect on those parts which remain compact, nor has the air the least power of acting on the blood through the liquor.

I had imagined, that since black blood contains more phlogiston than red blood, that difference would have appeared in the *air* produced from them, either by being simply dissolved in spirit of nitre, or when dried and made into a paste with that acid. But the difference was too small to be sensible to this kind of test. For this purpose, however, I had some blood drawn from the vein of a sheep, and also took some that came first after killing it, as the butchers usually do, by dividing the carotid artery; but though I dissolved the black part of the former, and the red part of the latter, in equal quantities of the same spirit of nitre, I found no sensible difference in the air that they yielded. The air that I got from them when dried, and made into a paste with spirit of nitre, was likewise equally indistinguishable. The quantity of air from this process was very great, and was produced irregularly, as I have observed it to have been when produced by a solution in spirit of nitre without drying. Half of this produce was fixed air, and the rest phlogisticated, except that a candle burned in it with a lambent blue flame. It is evident, however, from this experiment, that even the most florid blood con-

tains a considerable quantity of phlogiston; for, otherwise, this air would have been dephlogistated.

I would conclude this paper with observing, that I have found a very great difference in the constitution of blood with respect to its property of being affected by the influence of the air; some becoming very soon of a light florid colour, and the stratum of this colour soon growing very thick; whereas, in other cases, the colour of the blood in the most favourable circumstances, has continued much darker, and the lighter colour has never penetrated very far.

As the principal use of the blood seems to be derived from its power of receiving and discharging phlogiston, and the degree in which it possesses this power is easily ascertained by the eye, it might not, perhaps, be unworthy of being particularly attended to by physicians. To estimate the goodness of blood, according to this criterion, nothing is requisite but to observe the lightness of the colour, and the depth of the light coloured stratum, after it has been exposed to the air for a given time. In cases in which the blood is unusually black, and but little affected by common air, it should seem, that breathing a purer air might be prescribed with advantage.

In

In general, the blood that I have been able to procure in the city has not been so good as that which I have got in the country; owing, perhaps, to the cattle having been much driven, and heated before they were killed.

SECTION II.

Of the Consumption of dephlogisticated Air in Respiration.

WHEN I wrote the observations on the subject of *Respiration*, recited in the preceding section, I supposed that in this animal process there was simply an emission of phlogiston from the lungs. But the result of my late experiments on the mutual transmission of dephlogisticated air, and of inflammable or nitrous air, through a moist bladder, interposed between them, and likewise the opinions and observations of others, soon convinced me, that, besides the emission of phlogiston from the blood, dephlogisticated air, or the acidifying principle of it, is at the same time received into the

blood. Still, however, there remained a doubt *how much* of the dephlogisticated air, which we inhale, enters the blood, because part of it is employed in forming the *fixed air*, which is the produce of respiration, by its uniting with the phlogiston discharged from the blood. For such, I take it for granted, is the origin of that fixed air, since it is formed by the combination of the same principles, in other, but exactly similar, circumstances.

Dr. Goodwyn's very ingenious observations prove that dephlogisticated air is, as he properly terms it, *consumed* in respiration; but for any thing that he has noted, it may be wholly employed in forming the fixed air above-mentioned. He has proved, indeed, that the application of dephlogisticated air to the outside of a vein, will change the colour of the blood contained in it. But this might have been effected, as I first supposed, by the simple discharge of phlogiston from the blood, when it had an opportunity of uniting with the dephlogisticated air thus presented to it. He does not, however, seem to suppose that there is any phlogiston discharged from the blood in the act of respiration, but only that dephlogisticated air enters into it. But that my former supposition, as well as his, is true, will appear, I presume, from the experiments which I shall presently recite.

As,

As, in order to determine what proportion of the dephlogisticated air, destroyed by respiration, is employed in forming the fixed air which is the produce of it, it was necessary to ascertain as exactly as possible, the proportion of dephlogisticated air and of phlogiston, in the composition of fixed air, I repeated, with particular care, experiments similar to those which I had formerly made for that purpose.

I heated charcoal of copper in 41 oz. m. of dephlogisticated air, of the standard of 0.33, till it was reduced by washing in water to 8 oz. m. of the standard of 1.33. Again, I heated charcoal of copper in 40.5 oz. m. of dephlogisticated air, of the standard of 0.34, till it was reduced to 6 oz. m. of the standard of 1.76; and in each of these cases there was a loss of 6 grs. of the charcoal of copper, so that there cannot be more than 6 grs. of phlogiston in 33 oz. m. of fixed air; and consequently that only a very little more than one fourth of the weight of fixed air is phlogiston.

I heated perfectly well burned charcoal of wood, in 60 oz. m. of common air, and found one fifth of the remainder to be fixed air, and the residuum of the standard of 1.7. Lastly, I heated eight grains and a quarter of perfect charcoal in 70 oz. m. of dephlogisticated air, of the standard of 0.46, when it still continued 70 oz. m. but after washing in water, it was reduced

to

to 40 oz. m. of the standard of 0.6, and the charcoal then weighed one grain and a quarter; so that from this experiment with common charcoal, as well as from the preceding with charcoal of copper, it appears that about one fourth of the weight of fixed air is phlogiston, and consequently that the other three fourths are dephlogisticated air.

Having done this, I proceeded to ascertain how much fixed air was actually formed by breathing a given quantity both of atmospherical and of dephlogisticated air, in order to determine whether any part of it remained to enter the blood, after forming this fixed air.

For this purpose, I breathed in 100 oz. m. of atmospherical air, of the standard of 1.02, till it was reduced to 71 oz. m. and by washing in water, to 65 oz. m. of the standard of 1.45. When the computations are properly made, as directed in a former article, it will appear, that before the process this air contained 67.4 oz. m. of phlogisticated air, and 32.6 oz. m. of dephlogisticated air; that after the process there remained 53.105 oz. m. of phlogisticated air, and 11.895 oz. m. of dephlogisticated air, and that there were only 6 oz. m. of fixed air produced, for the quantity absorbed during the process could only have been very inconsiderable. It will therefore be evident, that, in this experiment, 20.7 oz. m. of dephlogisticated air, which would weigh

weigh 12.42 grs. disappeared; whereas all the fixed air that was found would only have weighed 4.4 grs. and one fourth of this being phlogiston, the dephlogistated air that entered into it would have weighed only 3.3 grs. consequently 9.12 grs. of it must have entered the blood, which is three times as much as that which did not enter, but was employed in forming the fixed air in the lungs.

I breathed in 100 oz. m. of dephlogistated air, of the standard of 1.0, till it was reduced to 58 oz. m. and by washing in water to 52 oz. m. of the standard of 1.75, with two equal quantities of nitrous air. The computations being made as before, it will appear that before this process this air contained 66 oz. m. of phlogistated air, and 34 oz. m. of dephlogistated air; and that after the process there were 30.368 oz. m. of phlogistated air, and 21.632 oz. m. of dephlogistated air. In this case, therefore, the dephlogistated air that disappeared was 13.3 oz. m. weighing 7.8 grs. and the fixed air was 6 oz. m. weighing 4.4 grs. so that here also about three times as much entered the blood, as did not.

These experiments I repeated many times, and though not with the same, yet always with *similar* results, the greater part of the dephlogistated air, but never the whole, passing the membrane of the lungs, and entering the blood.

When

When the results above-mentioned are compared, it will appear, though the observation escaped Dr. Goodwyn, that part of the phlogisticated air entered the blood, as well as the dephlogisticated air; or which is the same thing, that the dephlogisticated air which was consumed was not of the purest kind. This experiment I repeated so often, and always with the same result, that I am confident I cannot be mistaken in this conclusion. This fact, of which I had no previous expectation, I first thought might be accounted for by supposing that the two constituent parts of atmospherical air, viz. the phlogisticated and dephlogisticated air, are not so *loosely mixed* as has been imagined, but rather that they have some principle of *union*; so that though they may be completely separated by some chemical processes, they are not entirely so in this; but that the dephlogisticated air passing the membrane of the lungs, carries along with it some part of the phlogisticated air with which it was previously combined. But at the obliging suggestion of Dr. Blagden, I now think it more probable that the deficiency of phlogisticated air was owing to the greater proportion of it in the lungs after the process than before.

When I breathed dephlogisticated air that was very pure, I generally found less loss of phlogisticated air, and in one case (which is therefore not to be depended upon) there seemed to be an increase of it.

There

There will always be some uncertainty in the results of the long continued respiration of any kind of air, as at the last the operation becomes laborious, and the quantity inspired and expired is therefore much greater than at the first. But I was aware of this circumstance, and endeavoured to obviate the effects of it, by leaving off with my lungs, as nearly as I could judge, in the same state of distention as when I began, which was always after a moderate expiration; so that two or three ounce measures would have made a very sensible difference, as any person will find by actual trial.

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SECTION III.

Of the Respiration of Fishes.

HAVING discovered, as I apprehend, that the principal use of the *lungs*, and of the *blood*, is to discharge phlogiston from the animal system, in a state proper to diminish respirable air, and to render it unfit for respiration, I was willing to try whether *fishes*, which do not breathe as other animals, part with phlogiston to the water in which they live, and with that view I put two fishes (a pretty large perch and an eel) into a pail of water; and when they had been in it about twenty four hours, I nearly filled a large phial with it, and in it I agitated a small quantity of common air between six and seven minutes, and then found that it was considerably injured by the operation. Two measures of this and one of nitrous air occupied at first the space of two measures and one sixth, and by standing several days was never less than two measures. But when I agitated an equal quantity of air in the same quantity of the same water, in which no fishes had been confined, and for the same space

of time, it did not appear to have been injured except in the slightest degree.

It is evident, therefore, that phlogiston is discharged from fishes as well as from other animals, that this phlogiston affects the water, and that this water affects the air that is agitated in it; and in the same manner as the fishes themselves would have affected it, if it had been possible for them to breathe it.

I afterwards repeated these experiments with an attention to more circumstances; and they both confirm and extend my former general conclusions.

Having at hand some water from the Hot-well at Bristol, which I had found to contain air in a state of great purity, I completely filled a large phial with it, and I put into it a few very small fishes, which I had provided for the purpose of these and other experiments. They were minnows, and other small fishes, about two inches in length. In this water they were confined without any access of common air till they died.

After this I took equal quantities of the water in which the fishes had died, and of that out of which it had been taken, when they were confined in it; and I expelled from both all the air which they would yield. That from the water in which no fishes had been put, exceeded in quantity that from the

the water in which they had been confined in the proportion of three to two; and examining the quality of both these quantities of air, by the test of nitrous air, the former exceeded the latter in a still greater proportion. The air from the water in which no fishes had been confined was about the standard of common air, but that which had been contaminated by the respiration, as I may say, of the fishes, though not thoroughly phlogisticated, was something worse than air in which a candle just goes out. I should probably have found it still worse than this, if I had expelled and examined the air immediately; but the water remained in an open vessel all night before I made the experiment upon it.

From this experiment it may be concluded with certainty, that air contained in water, in an unelastic state, is as necessary to the life of fishes, as air in an elastic state is to that of land animals.

I had no doubt, therefore, but that putting fishes into water impregnated with air that was thoroughly phlogisticated, would be injurious, if not fatal to them, as much as the same kind of air, in an elastic state, is to land animals; and this was verified by the following experiments; from which, however, it appears that fishes, like insects, and some other exanguious animals, can live a considerable time without

without any thing equivalent to respiration. What limits that time has, may in some measure appear from these observations.

I began with water that contained, as far as we are able to discover, no air at all. For it was rain water, that had been recently boiled a considerable time. The vessel contained about three pints of it; and into this, without admitting any air, I put nine of the small fishes above-mentioned, and they lived in it between three and four hours. This experiment resembles the putting of frogs and serpents into a vacuum, only that there was no expansion of air contained in them, to swell their bodies in this case.

Taking the same water, which as I observed, contained little or no air, I made it imbibe as much as I could of a quantity that had been phlogisticated with iron filings and sulphur, six months before. Of this, however, the water would take but very little. Into a pint of this water, thus imperfectly impregnated, I put two of the fishes, and they lived in it near an hour. The result was the same when I impregnated an equal quantity of the same water with inflammable air. For in this case also the two fishes lived about an hour. This experiment resembled the putting of mice, and other land animals, into phlogisticated or inflammable air, which

is known to be fatal to them, but more suddenly than this water was to the fishes, owing, I suppose, to its imperfect impregnation.

That excellent anatomist, Mr. John Hunter, told me, that fishes would not live in water impregnated with fixed air. I repeated the experiment, and found that small fishes would not live in this kind of water more than a few minutes. At the same time I had the curiosity to try how they would be affected by water impregnated with *nitrous air*, and observed that they were affected in the same manner, but much more violently; being thrown into the greatest agitation the moment they were put into it, and moving about with the greatest rapidity, till they became languid and died.

Though at that time I took all the care I could to prevent the decomposition of the nitrous air, that remained after the operation, filling the phial in which the process was made with fresh water by means of a funnel, &c. still a decomposition of some small part of it would necessarily be made, before I could possibly slip the funnel into the neck of the phial. To prevent this, I now introduced the fishes into the vessel in which I had impregnated the water while it remained inverted in the basin, the remainder of the nitrous air not imbibed by the water,
still

still resting upon it. The phial I used contained something more than a pint, and the nitrous air occupied about one fourth of it.

Into this vessel, thus prepared, I introduced two of my small fishes, and they continued very quiet, without being seized with any convulsions, ten minutes, or a quarter of an hour, before they died. The cause of the convulsions, therefore, in the former experiment, must have been not the *nitrous air*, properly speaking, but the *nitrous acid*, though in so very small a quantity, diffused in the water, and acting like the fixed air (which is only another kind of acid) in the water impregnated with it. Whereas in this experiment the fishes were no otherwise affected than they were in the water impregnated with phlogisticated or inflammable air, except that the water imbibed much more of the nitrous air, and on that account was sooner fatal to them.

SECTION IV.

Of the Diminution of Nitrous Air in Consequence of its being confined in a Bladder in certain Circumstances.

AS the observations I made on the action of one kind of air on another, through a moist bladder, greatly illustrate the experiments in which the blood is acted upon through the membrane of the lungs, I shall insert them in this place. The composition of fixed air, as inferred from this process, was mentioned before.

Having had frequent occasion to put a quantity of nitrous air into a *bladder*, in order to transfer it from my trough of water to vessels placed in different parts of the room, I generally left the bladder with the remainder of the air in the trough of water, without pressing it out into a jar, especially when I knew that I should have occasion for it soon afterwards; and the trough being large enough, it was no inconvenience to me to let the bladder of nitrous air be swimming about in it, while I was intent upon other experiments. But several things led me to suspect, that nitrous air kept in a bladder, in these circumstances, was liable to

be impaired ; and it did not answer my purpose in a variety of experiments so well, as that which had been kept much longer in the jars, in the same trough of water.

As to the *quantity* of the air, I had not, for a long time, given much attention to it ; till sometimes finding it much less than I expected (the bladder evidently containing but little, when I perfectly recollected that I had left a good deal in it) I was forced to attend to that circumstance ; and after I had, in some measure, investigated this curious fact, I was a long time exceedingly at a loss to account for the capital circumstances of it.

I had the less suspicion of nitrous air undergoing any considerable change in a bladder, from my having at one time kept a quantity of it in a dry bladder about three weeks, without any sensible diminution of its virtue.

After this previous information, I shall now proceed to present my reader with a view of the facts, just as they occurred to myself.

On the 10th of March, 1776, I could not help observing that a large quantity of nitrous air, which had been left in a bladder, was greatly diminished ; it having been left very buoyant, and being now almost sunk under water. This prompted me to examine the state of the air contained in it, when I found it

to be mere phlogisticated air, not at all affecting common air, and extinguishing a candle. Now, as part of the same nitrous air had stood all the time in a glass jar, in the same trough of water, without suffering any sensible change, the difference between them was evidently owing to the different manner in which they had been kept.

In order to determine by what *degrees* this undoubted diminution of nitrous air, when kept in a bladder, would proceed, I put a certain quantity of it into a bladder, and let it swim about my trough as usual; when I found that, in the space of one day, it was diminished one fourth of its bulk, and the next day only half of it remained, when it was still considerably nitrous. Had I continued it longer, the remainder would certainly have been phlogisticated air, as before.

In order to determine whether this remarkable effect was necessarily occasioned by the *bladder* in all circumstances, I thrust a bladder up into a jar containing nitrous air; but, though it had sometimes its *inside*, and sometimes its *outside* in contact with the air, and was kept there a sufficient time, no change was produced in the air. But when, immediately upon this, I transferred the air into the same bladder, and let it swim about the trough, as usual, the change was effected as quickly as before; and

and being in a short time diminished to one fourth of its bulk, it appeared to be nothing more than phlogisticated air.

To try the effect of the bladder in another manner, I filled one quite full of nitrous air, and, tying it up, confined it under the shelf of my trough, where it was kept entirely covered with water; but though it continued in this situation a whole week, the quantity was not diminished, nor was the quality of it sensibly changed; at least not more than would have been the case if it had been kept in the jar the same length of time. At the same time I had filled another bladder with nitrous air, and tying it up, had left it with its neck downwards in a small quantity of water, so that by far the greatest part of the bladder was exposed to the common air. The quantity of this air was impaired a little. Then, without untying either of the bladders, I left them both to swim about in my trough, and in two days only the air in both of them had nearly lost its property of diminishing common air; but still that which had been kept covered with water retained rather more of its virtue than the other.

Since nitrous air had kept very well when it was confined there in a *dry* bladder, or one that was constantly *wet*, I now concluded that the diminution of the air in the bladder swimming about in the trough

must have been occasioned by that part which, in this situation, must have been exposed to be alternately wet and dry, or only partially moist.

To determine this, I filled two bladders quite full of nitrous air, and, in order to make a little variation in the experiment, had one of them wetted in the inside, and the other quite dry; and during two or three days I wetted them two or three times a day, suffering them to become quite dry at intervals. After this time they both appeared to be shrunk to about one fourth of their dimensions, and the air within them was become quite phlogisticated. At the same time another bladder wetted in the inside when it was filled, but not wetted any more, continued fully inflated three weeks; and being then examined, the quality of the air it contained was found to be very little impaired. But I was not aware that if the bladder had been kept constantly moist, the same effect would have been produced in much less time.

Being now satisfied with respect to the circumstances in which the nitrous air had been diminished in my bladders, I wished to ascertain the manner in which the *water* was affected by that decomposition, and I soon found that it had acquired a considerable quantity of phlogiston. For this principle was readily communicated to the air contained in the jars that

that stood in the same trough of water, and had injured the air contained in them, in proportion as they had been more or less exposed to its influence.

For example, the air contained in several jars standing in the trough of water, in which was swimming a large open bladder filled with nitrous air, and which, as it subsided by the diminution of the air, had been kept supplied with fresh nitrous air from time to time, I found to be, in all of them, more or less injured, and in those most of all that were most exposed to the body of water in the trough; as particularly in one jar that was so poised as to swim about in it, and another that was placed with its mouth half over the shelf. As to those that stood wholly on the shelf, the water in the inside of them had but little communication with the phlogificated water in the trough, and in them the air was but little affected.

It was still more evident that the water, in contact with which nitrous air is thus decomposed, was become very *acid*.

Having put rain water into a bladder, and filled it up with nitrous air, I tied it up very close, and left it to swim in a basin of water, taking care that the mouth of it was always under the water. After a week, perceiving that it was shrunk up to about one fourth of its bulk, I opened it, carefully pressing out the water into a clean phial; when I found the air
within

within it thoroughly phlogisticated, not affecting common air in the smallest degree, and the water was exceedingly acid, very turbid, a little inclined to yellow, and presently made a deposit of the same colour.

This result proves what I was not aware of at the time, viz. that the acidifying principle in the atmospheric air, had communicated through the bladder with the nitrous air within it, and by that means formed *nitrous acid*, while the phlogiston of the nitrous air had, in like manner, been communicated to the acidifying principle on the outside of the bladder, and with it had formed *fixed air*, as I observed afterwards. The same was the effect when inflammable and dephlogisticated air were separated by a moist bladder, as has been seen.

B O O K X.
EXPERIMENTS AND OBSERVATIONS
RELATING TO SEVERAL SUBSTANCES
CONTAINING PHLOGISTON.

P A R T I.
EXPERIMENTS ON CHARCOAL.

S E C T I O N I.
Experiments and Observations on Charcoal, first published in the Philosophical Transactions, Vol. LX. p. 211.

AMONG the original experiments, published in the History of Electricity, was an account of the conducting power of charcoal. This substance had been considered by electricians, in no other light than that of more perfectly baked wood, which is known to be no conductor of electricity. I have even heard of attempts being made to excite it; and
though

though those attempts were ineffectual, the failure of success was attributed to other causes than that of charcoal being no electric substance; so fixed was the persuasion, that water and metals were the only conducting substances in nature. The consideration of the chemical properties of charcoal, which are, in many respects, remarkably different from those of the wood from which it is made, might have led philosophers to suspect, that since, after its being reduced to a coal, it was become quite *another thing* from what it was before, it might possibly differ from wood in *this* property; but this consideration had not been sufficiently attended to.

In the account of my former experiments, I observed, that there were very great differences in the conducting power of charcoal, and particularly of wood charcoal, though I could not determine on what circumstances in the preparation, &c. those differences depended. I therefore expressed a wish, that some person, who had conveniencies for making chemical experiments, would prosecute the inquiry, as one that promised, not only to ascertain the cause of the conducting power of charcoal, but perhaps of *conducting power universally*. Not hearing that any chemist, or electrician, has attended to this business, I have, at length, resumed the subject, though not with every advantage that I could have wished,

wished. I have, in a great measure, however, succeeded in the principal object of my inquiry; and I shall now lay before this society the result of my experiments and observations.

I shall begin with correcting a mistake I lay under at the time that I made the former experiments. Having been informed by persons, who attend the making of *pit charcoal*, that it was considerably increased in bulk after the process; I imagined that all other substances received an increase of bulk, when they were reduced to a coal; but the first experiments that I made, convinced me of my mistake. All vegetable substances are considerably contracted in all their dimensions, by the process of coaling, and the more perfect this process is (that is, as will be explained hereafter, the greater is the heat that is applied in the course of it) the greater is the diminution. I have even reduced pieces of wood to little more than one fourth of their original length and breadth, in a common fire, by the use of a pair of hand-bellows only. And this was the case equally with wood of the firmest texture, as ebony; that of a middle texture, as oak; and that of the loosest, as fir, &c.

As moisture (and, I believe, small degrees of heat or cold) affects wood much more sensibly *across* the fibres than *along* them, it might have been supposed, that when wood was reduced to a
coal

coal by the application of a greater degree of heat, the same rule would have been observed; but I found very little difference in this respect. To ascertain this circumstance, I took from the same board, two pieces, each two inches and a half in length. In one of them, the fibres were divided, in the other they were not; and after coaling them thoroughly together, in the same crucible, I found that the former measured 2.05 inches, and the latter 2.15. Their conducting power could not be distinguished.

A more particular account of the degree, in which wood is shortened in coaling, will be seen afterwards, when the variations in this respect are compared with the variations in the power of conducting electricity.

To my great surprize, I found *animal substances* not reduced in their dimensions by the process of coaling. This, at least, was the case with some pieces of *ivory*, several inches in length, and a piece of *bone*. They bore a very intense heat for many hours, and came out of the crucible considerably diminished in weight, but hardly so much as distorted in their shape, as is remarkably the case with wood, and, I believe, all vegetable substances.

In examining mineral substances, I found that my information, mentioned above, was just. Coals are very much enlarged in their dimensions by

charring; but the experiment must be made with great care, to judge of this circumstance; for, unless the operation be very slow, the coal will retain nothing of its former shape, having been made, in some measure, fluid by the heat. The inside of all pieces of pit-charcoal is full of cavities, and there is generally a very large one in the centre of every piece; so that the dilatation is nothing like the extension of fibres; but is produced by the elasticity of the new-formed vapour, in forcing its way out, while the substance is soft.

With respect to the main object of my inquiry, I presently satisfied myself, that the conducting power of charcoal depends upon no other circumstance than the *degree of heat*, that is applied in the process of making it. I had not suspected this; but numberless experiments clearly proved it. Taking an iron pot, filled with sand, and putting into it pieces of wood, cut out of the same plank, marking them, and carefully noting their places in the pot, I always found that those pieces came out the best conductors, that had been exposed to the greatest heat. The result was the same when I made coals of bits of wood, placed one above another, in a gun-barrel, one end of which was made red hot, and the rest gradually cooler and cooler.

Taking pieces of charcoal that conducted very imperfectly, or not at all, I never failed to give them
them

them the strongest conducting power, by repeating the process of coaling, either in a crucible, or in a gun-barrel, covered with sand, and kept in an intense heat.

I could not find that the mere continuance of the same degree of heat had any effect with respect to the conducting power of charcoal.

Mr. Macquer, and other chemists, define charcoal to be *wood burned, without being suffered to flame*; but, with respect to its conducting power, and, I make no doubt, with respect to all its other essential properties also, it makes no difference whether it flame or not. I have coaled pieces of wood, both in gun-barrels, and in crucibles, slightly covered with sand, and have let the inflammable vapour that exhaled from them take fire, at various distances from the substances; and I have also put pieces of wood in an open fire, and urged the heat applied to them, with a pair of bellows; and in all these cases have found the charcoal equally good. In the last method, indeed, very little of the substance is preserved; but the little that doth remain, after it hath ceased to flame, whether it be quenched immediately, or not, conducts as well as any charcoal whatever. But we can hardly be sure that the same degree of heat is given to every part of a piece of wood, except it be exposed to it for some time; and in an open fire, urged with a pair of bellows,

bellows, the wood wastes as fast as it is red hot, before the center of it is much affected with the heat.

When once any degree of conducting power is given to a piece of charcoal, I never found that it was afterwards lessened. A partial consuming of it in an open fire doth not affect the remainder, as I observed in the account of my former experiments.

I had imagined, that the *solidity* of substances converted into charcoal, would have had a very considerable effect on their conducting power afterwards; but the conjecture was not confirmed by experiment. Coals made of the lightest woods conducted, as far as I could perceive, as well as those that were made from the most solid, if they had been exposed to the same degree of heat in the process. Fine shavings of fir, the fine coats of an onion, the lightest foot, and every other vegetable substance that I tried, conducted equally with coals made of oak or ebony.

I had imagined, also, that the moment a piece of wood was become black with heat, it was, to all intents and purposes, a real charcoal; and, along with the other properties of charcoal, would conduct electricity, more or less; but I found, by coal-ing several pieces very slowly, that they would not conduct in the least degree, not only when they were

made superficially black, but likewise when they were black quite through, and had remained a long time in the heat that made them so; so that no eye could distinguish them from the most perfect charcoal.

I have sometimes found charcoal in such a state, that it would assist the passage of an explosion along its surface, when it would not conduct a shock any other way.

In order to satisfy myself in what proportions the *diminution of weight*, the *decrease of bulk*, and the *conducting power* of wood and charcoal, corresponded to one another, I took several pieces from the same plank, and having carefully weighed and measured them, converted them into coal very slowly, and by a gradual increase of heat, on an iron plate, held on the fire, turning them constantly, to prevent their catching fire. The following were the results.

A piece of very old dry oak, weighing twelve grains, and which conducted in the imperfect manner that wood generally does, from the moisture it contains, was, after the loss of about one grain, no conductor at all; and it continued the same as baked wood, till it was reduced to four grains, when it was black quite through; and even then, no part of it conducted, except one corner, where it had caught fire.

Another

Another piece I carefully weighed, and measured several times in the course of the process. At first it weighed

Gr.	Length. Bread. Thick.
12	its dimensions in inches were 2.45 .12
At 8	2.4 .12
— 5.5	1.91 .4 .12
— 3.5	1.8 .35

It was now become an imperfect conductor. I then urged it with a strong heat, in a crucible, and taking it out, it weighed 1.75 gr. and measured 1.6 in length, and .3 in the other dimensions. It was now a perfect conductor; and though I afterwards kept it in a very intense heat several hours, by which it was reduced to one grain in weight its conducting power was not sensibly increased; but it was become very brittle, or friable.

It appears from these experiments, that these pieces of wood were reduced to about one fourth of their weight before they would conduct at all; though, at the same time, they were diminished in length (*i. e.* along the fibres) only one tenth. The breadth and thickness could not be measured with sufficient accuracy in these small pieces. To make them perfect conductors, they were reduced to about one tenth in weight, and one half in length.

A variety of circumstances led me to conclude, that the cause of *blackness*, and of the conducting

power in charcoal, is the oil of the plant, made empyreumatic, and burned to a certain degree. I therefore conclude that these properties are some way connected with that part of the inflammable principle, otherwise called phlogiston, that is fixed and united to the earth of the plant, when the union is strengthened by an intense heat.

The *sand*, with which I covered the substances that I converted into coals, and also the *pipe-clay* which I sometimes put over them, contracted a blackness like charcoal, and would often conduct pretty well. Sometimes they would conduct a shock. This must have been owing to the oil they received from the substances out of which it was expelled by the heat. In the experiment of the gun-barrel filled with pieces of wood, mentioned above, the uppermost pieces were not in the least burned. They could hardly have been hot; yet, having contracted a superficial blackness, from the vapour of the oil expelled from the piece below them, they would even conduct a shock, though not in the most perfect manner.

Sometimes those substances that had no phlogiston themselves, but received it in consequence of being placed in the neighbourhood of other bodies out of which it was expelled, would not conduct immediately; but would be made to do so by being exposed to a greater heat, which more thoroughly

roughly burned the oil with which their pores were filled.

I put a piece of a common *pipe* into a crucible, in which I was burning some turpentine (which will be mentioned below) and it came out black quite through, like a pipe in which tobacco has been frequently smoked. In this state it would not conduct at all; but putting it into a crucible, covered with sand, I treated it in the same manner as I would have done a piece of wood, in order to coal it, and it came out a very good conductor. Had it been burned in the open fire, the phlogiston would have escaped, and the pipe would have been left white, as at first.

Being convinced that the conducting power of charcoal depended upon the oil, or rather the phlogiston contained in the oil, and on the degree of heat with which it was burned, I took several methods to give vegetable substances more of this principle; or at least endeavoured to make them retain more of it than they usually do, in the process of coaling. But I had no apparent success in those experiments.

I began with plunging a piece of old dry oak in oil; and then, pumping the air out of it, let it stand *in vacuo* a day and night, in which time it seemed to discharge a great quantity of air; after which I let the air into the receiver, and thereby forced the

oil into its pores. But the coal from this wood was not sensibly better than others. The application of heat may, perhaps, expel the phlogiston in such a manner, that the residuum, being fully saturated, can retain no more than a certain proportion. I made coals of other pieces of wood, when they were covered with cement; and I also coaled several pieces together, that they might receive phlogiston from one another; but, in both cases, without any sensible improvement in the quality of the coal.

In order to prevent the escape of the phlogiston belonging to the substance to be reduced to a coal, I put some pieces of wood into a gun-barrel, and corked it as close as I could, at the same time covering the cork with cement. In this case the rarefaction of the exhaling vapour never failed to drive the cork out; but it must have been after a considerable resistance to its escape. However, I could not perceive any peculiar excellence in the charcoal made in this manner.

I do not, indeed, know any method in which differences in substances that conduct so well as these can be accurately tried, at least none that can be applied in this case. The charcoal I can make in a common fire, by the use of a pair of hand-bellows, I cannot distinguish, with respect to its conducting power, from the most perfect metals, gold
and

and silver; either by the length of the electric spark, the colour of it, or the sound of the explosion. I make no doubt but that wood, in the process of coaling, may easily have a degree of conducting power communicated to it, exceeding that of lead, iron, or the other more imperfect metals.

We may, perhaps, be guided in our conjectures on this subject, by considering the *degree of heat* that is necessary, either to unite the phlogiston to its base, or to separate them, both in the case of wood, and the different metals. Lead is very easily calcined, and it is also known to conduct electricity very imperfectly. Iron soon turns to rust; and its conducting power I found to be very small, in comparison with that of copper, or the more perfect metals. If, therefore, in making charcoal, a degree of heat be applied greater than is necessary to calcine or revive a metal, we may perhaps conclude, that the conducting power of the charcoal will be superior to that of the metal. As it may be possible to give charcoal, when cut off from any communication with the external air, a greater degree of heat than silver or gold would bear without being dissipated in vapour; it may even be possible to make charcoal that shall conduct electricity better than those most perfect metals.

Had there been any phlogiston in water, I should have concluded, that there had been no conducting power in nature, but in consequence of some union of this principle with some base. In this, metals and charcoal exactly agree. While they have phlogiston, they conduct; when deprived of it, they will not conduct*.

I believe, however, that all vegetable or animal substances, that contain phlogiston, may be reduced to a coal; and if the heat applied in the process be sufficient, that coal will conduct electricity. Flesh, glue, bones, and other parts of an animal body, make good conducting charcoal.

The only approach, or seeming approach, I ever made towards retaining more phlogiston than usual, in wood reduced to a coal, was by the *slowness of the process*. For I always found, that if the heat was applied very gradually, less volatile phlogiston, *i. e.* less inflammable air was expelled; and therefore I suppose that more of it was fixed. I could never afterwards, by equal degrees of heat, make this coal to weigh as little as another that was first coaled by a sudden heat. This agrees with those experiments in which more air was got from various

* As water attracts dephlogisticated air from the atmosphere, which is the property of all substances containing phlogiston, it is probable from this circumstance, as well from its conducting electricity as metals do, that like them it also contains phlogiston.

substances by a heat suddenly applied, than by the same degree of heat applied more slowly.

I took two pieces of dry oak, the contiguous parts of the same stick, each weighing exactly fourteen grains. One of these I heated suddenly. It yielded eight ounce measures of inflammable air, and then weighed two grains. The other I heated slowly, but as vehemently, at the last, as the other. It yielded only one ounce measure and a half, and weighed three grains.

I repeated the same experiment several times, and always with nearly the same result.

Examining the conducting power of the pieces of charcoal made with these different circumstances in the process, I could not distinguish which was better. Perhaps a more accurate method of trying them might show, that those which were coaled slowly were the better conductors; unless, which is not improbable, the goodness of the conducting power consists in the *completeness of the union* that is produced between the inflammable principle and its base, which will depend upon the *degree of heat only*, and not on the *quantity of phlogiston* thus united to the earth.

N. B. To catch the inflammable air, set loose in making charcoal, I put the substances into a gun barrel, to which I luted a long glass tube, and to the

the tube I fastened a bladder, out of which the air was carefully pressed.

As metals and charcoal agree in consisting of phlogiston united to an earthy base, and also in conducting electricity, I suspected that these two different substances might also agree in their readiness to expand by heat. Mr. Smeaton was so obliging as to assist me in my attempts to ascertain this circumstance, by the application of his excellent pyrometer. Though we could not make the experiment with all the exactness that we could have wished, yet the result of near thirty trials was uniformly in favour of the greater degree of expansion, by heat, in the charcoal, than in wood of the same kind (as we imagined) out of which it was made. In general, the expansion of the charcoal was about double to that of the wood.

It is evident, that a certain degree of heat makes wood and charcoal expand; and also that a greater degree of heat makes them contract. I wish we had an instrument to ascertain the precise degree of heat, at which the expansion ceases, and the contraction begins; and whether the two effects be produced by the same gradation.

In the course of these experiments on charcoal, I met with a substance, the conducting power of which is singular, and exhibits a beautiful appearance

ance. In order to see what would remain after burning a quantity of turpentine in a glass tube, I covered it with sand, in a crucible, in the same manner in which I used to make charcoal; and, after letting it continue a sufficient time, in a very hot fire, and after the flame had long ceased, I examined the tube, and found that it had been melted; but, instead of any thing like charcoal, or the least blackness, I observed that the tube was uniformly lined with a *whitish glossy matter*, which I could not scrape off. Upon trying whether it would conduct electricity, I found it transmitted the smallest shocks to a considerable distance; and, what appeared very remarkable, the path of the explosion was luminous all the way, and seemed to consist of a prodigious number of small separate sparks, scattered to a great distance, exhibiting such an appearance as would be made by firing gunpowder scattered carelessly in a line. The explosion very much resembled the firing of a squib. To compare it to another electric appearance, it was like the explosion passing over a thin surface of gilding.

I imagine that, though I could not perceive any interruption in this white coating, not even by the help of a microscope, it must, in fact, have been full of interstices, and the electric sparks could only be visible in passing from one conducting particle to another.

In this experiment, I often got pieces of glass very imperfectly covered, with intervals in the white coating very large and visible; but, though I exposed the same pieces of glass to catch more of this matter, I never could get a coating of it so thick, but that, in transmitting the electrical explosion through it, it exhibited the same luminous appearance, as if there were interstices in the circuit.

I got the same matter from oil of *turpentine*, and oil of *olives*; but not from *bees-wax*, or *spermaceti oil*. Perhaps it cannot be got from any animal substance.

In order to observe the progress of this incrustation, I poured oil of *turpentine* on some flat pieces of glass, and burned them on an iron plate, in the open fire, the heat being moderate; but the effect was a black covering, like soot, which would not conduct in the least. But these same pieces of glass, thus covered with the black coating, being put into a crucible full of sand, and urged with a strong heat, came out white, and conducted exactly as before.

With a less degree of heat, the black covering was changed to white; but it did not adhere so firmly to the glass as when the heat had been greater; though it adhered more closely than the black covering, which might be wiped off with a feather. But this white coating, produced by a moderate heat, would not conduct at all.

In

In some cases I have found this whitish matter to be dispersed by several explosions, as Dr. Franklin found gilding with leaf-gold to be.

In whatever manner the pieces of glass were covered, the coating vanished when it was made red hot in an open fire; and the glass that remained would not conduct, any more than it did before. This circumstance exactly resembled the escape of phlogiston from charcoal and metal, burned in the open air.

In a microscope, this whitish matter looked exactly like metal, or rather some of the semi-metals, having a bright polish, though it soon became, as it were, tarnished.

To try whether it was metal, I dipped the pieces of glass that were covered with it in the *acids*, but found that they had little or no effect upon it, though it is by no means fixed in the pores of the glass, but covers it quite superficially.

It was not in the least affected by the *magnet*. Upon the whole, the matter that forms this coating of the glass seems to be a kind of charcoal, only white, instead of black.

SECTION II.

Experiments on Air from Charcoal.

THE examination of *charcoal* is now considered as an object of peculiar importance in chemistry; and ever since I had discovered it to be one of the best conductors of electricity I have given particular attention to it. The most important of the experiments that I have made respecting it since my more early publications, will be found in the account that I have given of the decomposition of it by steam in a state of ignition. In this section I shall comprize a variety of miscellaneous observations, some of more, and others of less importance. Some of them also are repetitions of former experiments, but made with a better apparatus, and with an attention to more circumstances.

The quantity of air to be expelled by heat from dry wood I have frequently estimated; but having now, by the assistance of Mr. Wedgwood, the advantage of doing every thing of this kind in very compact earthen retorts, which themselves give no air, it may be worth while to mention, that from five ounces of

dry oak I got 650 ounce measures of air, of which about one half, nearly a third, that came at first was fixed air, the remainder being inflammable, and the last portions wholly so.

The property that charcoal has of absorbing air is a remarkable circumstance, first distinctly observed by the Abbé Fontana; but still there are several particulars relating to this experiment, such as the *time* in which the air is imbibed, and the quality of it when it is again expelled by heat, &c. that are not undeserving of notice. And though I have not pursued this subject with much regularity, I have occasionally, and at different times, made observations of this kind, of which I shall here give an account; proposing to resume the experiments, and to pursue them farther.

From 789 grains of charcoal, from which all air had been expelled, and which had been exposed to the atmosphere, I got thirty ounce measures of air, no part of which was fixed air, but all phlogisticated, extinguishing a candle; being of the standard of 1.7. Probably pure air had been imbibed in preference to any other; because when it has been made to imbibe dephlogisticated air only, it comes out again partially phlogisticated. I found, however, considerable varieties in the quality of air emitted by charcoal, after being exposed to the open
air,

air, as well as other circumstances of some consequence relating to the experiment.

From 680 grains of charcoal, which had been heated four times before, I got forty ounce measures of air, of which the slightest portion imaginable was fixed air. Of the rest, towards the middle of the process, the standard was 1.48, and the last, 1.52. The next day, without changing the retort, or moving it from its place, having only left it with its mouth open, to give it an opportunity of attracting more air, I heated it again, and got about two ounce measures of air, of the standard of 1.5. This was much less than I had expected. I then tied a bladder to the mouth of the retort the moment it ceased to give air. But though the charcoal was shaken out of the retort into the bladder when it was cold, very little air had been absorbed by it. It then weighed 560 grains. But having been thus exposed to the open air, though for a short space of time, on being again immediately subjected to heat, it gave fifty ounce measures of air, the standard of which varied in different periods of the process, in the following order: 1.54, 1.58, 1.7, 1.6, and about one twentieth of the whole was fixed air. Why this same charcoal should give fifty ounce measures of air now, and only forty before, I cannot tell. Probably a little moisture had been attracted by it.

Being

Being willing to ascertain the weight that was gradually gained by charcoal, in consequence of being exposed to the open air; on the 4th of September I left in an open dish charcoal fresh made from dry oak, weighing

-	364 grains
The next day it weighed	- 390
Two or three days after,	- 397
The 24th of October following	419
The 26th of April	- 421

It appears, therefore, that charcoal fresh made, only absorbs about half as much air on its first exposure to the atmosphere as it does in a course of time afterwards.

Judging that this charcoal would not now acquire any more weight, I subjected it to heat in an earthen retort; and having got from it a quantity of air that was considerably phlogisticated, found that it weighed 312 grains, but the retort appeared to be cracked.

Having left the same charcoal exposed to the open air a whole year, I weighed it again, and found it to be 371 grains. That this charcoal should be reduced to less weight than it had been at the beginning of the process, I could not account for at that time; but I now do it by supposing that, together with *air*, some *moisture* had been imbibed; and this would help to decompose the charcoal, when it was subjected to the fire again, as is explained

in a preceding article. It may, however, be determined whether the air expelled from charcoal by heat, be the air which it had imbibed, or that which was formed by the decomposition of the charcoal by means of water. For this will be inflammable air, whereas the other, as appears by these experiments, will be partially phlogisticated.

Having gone through another process of exposing charcoal to air, and then expelling air from it by heat, I shall here note the particulars of it.

From two ounces of pounded charcoal, on the 20th of January, I expelled, by means of a strong heat, 336 ounce measures of air, and weighing it immediately afterwards, found it to be 756 grains. On the 23d it weighed 817 grains, after being exposed on a plate, so as to lie about half an inch in depth. The air expelled from it was about one tenth fixed air. This charcoal I exposed to the fire several times, the last time on the 28th of June in the year following, immediately after which it weighed 711 grains. Some of the air that I got from this charcoal was inflammable, burning with a lambent blue flame, which shews that moisture had been imbibed by the charcoal.

I have observed, that when charcoal has imbibed air, it will give it out again, at least in part, on being plunged in water, as well as by being exposed to heat. I again plunged into water pieces of charcoal,

charcoal, both perfectly and imperfectly made, after having been some days exposed to the open air, and found that the air they gave out in this way was in both cases common air. It is evident, therefore, that the degree of phlogistication in the air, expelled by heat, is owing to the decomposition of the charcoal.

Having repeated the Abbé Fontana's experiments, by introducing hot charcoal through mercury, into vessels containing different kinds of air, and being willing to recover by distillation the mercury that had been imbibed by the different pieces of charcoal, I at the same time took some notice of the air that came from them. The quantity of air was very considerable, but I took no exact account of it. With respect to its quality, it was partially phlogisticated, the standard of it being about 1.6, though the last that came was inflammable.

In my experiments relating to inflammable air, I made several on the decomposition of charcoal in the sun, the general results of which are there mentioned; but as the particulars were not many, I shall here recite them. It is only to be observed, that at the time that I made them I was under a mistake with respect to the origin of the air I procured, imagining it to proceed wholly from the charcoal; whereas I afterwards found that a degree of moisture, to which I had not then attended, was

necessary to the formation of that air. These experiments were all made by the heat of the sun in vacuo.

Four grains of charcoal treated in this manner, yielded twenty ounce measures of air, all inflammable, except that it barely made lime water a little turbid; but without any diminution that I could measure. At another time four grains of charcoal gave twenty four ounce measures of air, no part of which was fixed air.

I entirely dispersed one grain of charcoal in vacuo, and it gave six ounce measures of air, without the slightest appearance of its containing any fixed air. This charcoal had been long exposed to the open air, and on that account would give out more air than it otherwise would have done.

Four grains and a half of charcoal gave twenty two ounce measures and an half of air; and lastly, three grains and three quarters gave twenty three ounce measures and an half of air, without the least portion of fixed air in it.

I shall close this account with an experiment, in which I proceeded to take the specific gravity of the air which I got in these processes, without at the time drawing a very obvious conclusion from it. From about two grains and a half of charcoal of oak, I got fifteen ounce measures and a half of inflammable air, no part of which was fixed air, and

and weighing this air, I found that twenty ounce measures of it weighed four grains and a quarter less than the same bulk of common air. According to this proportion, the fifteen grains and an half of inflammable air ought to have weighed 5.96 grains, which is much more than the weight of the charcoal. But though I made this very observation at the time, I did not then infer, that *water* must enter into the composition of this air, having no suspicion that the water at the bottom of the receiver, several inches below the place on which the charcoal was exposed to the heat of the lens, could be attracted by it. Had not subsequent experiments shewn me the real nature of this inflammable air, this experiment must have remained inexplicable by me. At the time, I imagined, I believe, that the additional weight of the air was owing to the extraneous *water* which it had imbibed in being transferred from one vessel to another.

Having made the preceding experiments with charcoal from *wood*, I made a similar one with that from *pit-coal*, and I found that a piece of it, heated in vacuo, yielded four ounce measures of air, after having lost something less than a grain in weight. The air had in it a small quantity of fixed air, but the rest was all inflammable. The charcoal had been a long time exposed to the common air.

After the preceding experiments with charcoal, it occurred to me to vary them by making use of *foot*; but I was much surprized indeed, to find that it was a substance very different from charcoal, with which at first sight it might seem right to class it; for it contained a portion of pure air. A quantity of it being put into an earthen retort, and exposed to a strong heat, yielded air so pure, that with equal quantities of nitrous air, the test was 0.5, a degree of purity far exceeding that of common air. There was, however, some inflammable air mixed with it, which made it burn with a slight blue flame.

Taking some of this *foot* from which air had been expelled by heat, I again exposed it to the heat of a burning lens in vacuo, and from a grain and a half of it got six ounce measures of air, all inflammable, and burning with a blue flame, without any fixed air in it. It was, therefore, after the former experiment, a true *charcoal*, but not before.

Some experiments I made with *Homberg's Pyrophorus*, being similar to those made with charcoal, I shall add them here.

Homberg's Pyrophorus is a substance liable to be spoiled by exposure to the air. I prepared some of it, with a view to observe what it imbibed from the air, and with it I made the following experiments.

It

It was composed of two parts burned alum, one of salt of tartar, and one of charcoal. About two ounce measures of this pyrophorus I suffered to burn in the open air; and then in an earthen retort, I extracted from it 144 ounce measures of air, of which one half at the first was fixed air, but at the last very little. The residuum of the first portion extinguished a candle, but that of the last burned with a lambent blue flame. When examined with nitrous air, both the residuums were of the standard of about 1.8.

The pyrophorus was then kept two days in the retort, with its mouth in mercury, and then being taken out, it presently grew hot, and burned as well as ever. Immediately before the burning it weighed 428 grains, immediately after it 449 grains. Having been spread thin, and exposed to the atmosphere, the next morning it weighed 828 grains; but when well dried, it weighed only 486 grains. It was then subjected to a greater heat than before, and it yielded 110 ounce measures of air, the first portions of which were half fixed air; but the last contained very little, and burned with a lambent blue flame. The substance then weighed 396 grains.

Afterwards I took a quantity of pyrophorus, which would not take fire in the open air, and heating it in an earthen retort, found five sevenths of the first part of the produce fixed air; but this pro-

portion gradually diminished, till at last nine tenths of the whole was inflammable air, burning with a lambent blue flame. This inflammable air being decomposed with an equal quantity of dephlogisticated air, yielded 0.86 of a measure of fixed air.

Another quantity of pyrophorus, which had burned very well, and which, during the burning, and in two days exposure to the atmosphere afterwards, had acquired 132 grains in weight, being again exposed to heat in an earthen retort, gave an hundred and eighty ounce measures of air, of which the first portion was three sevenths fixed air, and the rest phlogisticated. But afterwards one half only was fixed air, and the rest inflammable, burning with a lambent flame, and at last it was wholly inflammable. When this pyrophorus was cool, it took fire again by exposure to the open air, but not without the assistance of some external heat. It had been red hot through its whole mass at the first burning of it, and had continued so a considerable time, after which the surface of it was slightly covered with white ashes; but all the inside was as black as ever it had been.

SECTION III.

Of the Charcoal of Metals.

HAVING transmitted *steam*, or the vapour of water, through a copper tube, I was willing to try the effects of *spirit of wine* through the same tube when red hot, as I had before procured inflammable air by sending the same vapour through a red hot tobacco-pipe. In this case, the vapour of the spirit of wine had no sooner entered the hot copper tube, than I was perfectly astonished at the rapid production of air. It resembled the blowing of a pair of bellows. But I had not used four ounces of the spirit of wine before I very unexpectedly found, that the tube was perforated in several places; and presently afterwards it was so far destroyed, that in attempting to remove it from the fire it actually fell in pieces. The inside was full of a black footy matter resembling lamp-black.

Upon this I had recourse to *earthen tubes*, and found, that by melting copper and other metals in them, and transmitting the vapour of spirit of wine in contact with them, different substances were

formed according to the metals employed. The new substances hereby formed may be said to be the several metals supersaturated with phlogiston, and may perhaps not be improperly called the *charcoal of the metals*.

That this appellation is not very improper, may appear from these substances yielding inflammable air very copiously when they are made red hot, and the steam of *water* is transmitted in contact with them, just as when the charcoal of wood is treated in the same manner.

In order to make farther observations on the nature of this process, I put the copper into an earthen tube, on which I had not found the vapour of spirit of wine to have any action, though itself was decomposed in passing through it, being chiefly converted into inflammable air.

In the first experiment I sent three ounce measures of spirit of wine over two ounces of copper, in a degree of heat that kept it just melted. This was attended with a copious production of such air as would have come from the spirit of wine only. But what surprized me most in the result was, that, though the copper had lost no more than twenty eight grains of its weight, I actually collected 446 grains of the charcoal; chiefly in the form of powder, though some of it consisted of large flakes, several inches long; having been separated at once
from

from the whole surface of the melted copper. These pieces bore handling without any danger of breaking, and were nearly quite black.

In another experiment I got 508 grains of charcoal from nineteen grains of copper. But this was from copper in thin plates, and they were not all converted into perfect charcoal, there being something harder, and therefore partially metallic in the centers of them.

Much of the charcoal was dispersed and lost in the fine black powder with which the air was loaded; and as in what I collected the copper was only about a sixteenth part of the composition, I think I may venture to say that, in reality, it was not more than a twentieth part. In this respect it resembles the charcoal of wood, or of pit-coal, in which the ashes bear a very small proportion to the inflammable air, or phlogiston, of which the bulk of the charcoal consists. This charcoal of copper was also as insoluble in acids as that of wood, and it likewise resembles it in other respects.

When the heat in which this process is conducted is great, the minute division, and volatility of this charcoal, is very extraordinary. Seeing it issue from the end of a tube in a dense black cloud, I endeavoured to collect it in a large glass balloon. But after having given the balloon an uniform, but very thin black coating, not distinguishable in its appearance

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pearance from *foot*, it issued from the orifice like dense smoke. I then connected with it several adopters, and other vessels, to which it gave a similar coating; and lastly, I plunged the tube out of which it finally issued, very deep in a vessel of water. But still the air came through the water loaded with the same dense smoke, and very little of the matter could be collected*. I was therefore satisfied, that the only way to collect any considerable quantity of it, was to make use of a degree of heat just sufficient to keep the copper red hot, or rather just melting.

Spirit of turpentine I found to answer as well in the production of this charcoal as spirit of wine. In this process I got 120 grains of charcoal from five grains of copper, notwithstanding a very dense black smoke with which the air was charged, and in which, no doubt, a great part of the charcoal was dispersed and lost.

Having procured this new substance, I proceeded to make several experiments upon it; and was much disappointed in finding that in common air it was only melted by the burning lens, and that the heat

* The incoercibility of this foot, in the form of vapour, very much resembles that of the dense vapour produced by the decomposition of inflammable and dephlogisticated air, of which an account was given before, both of them passing through water, without being retained by it.

had not (at least in a short time) any sensible effect upon it. But in dephlogisticated air (as I shall describe more particularly in the section relating to fixed air) it burned rapidly, and converted almost the whole of it into fixed air.

It will not be thought surprizing, that no sensible effect should be produced by heating this substance in inflammable or in alkaline air. But being made red hot in the latter, the air was increased in bulk, and a considerable part of it became inflammable, as it would have been by heating any thing else in it.

Considering this substance as a *charcoal*, and being engaged at the time of my discovery of it, in making steam of water pass over charcoal of wood, confined in red hot earthen tubes, I treated a quantity of this charcoal in the same manner, and the result was such as might have been expected. A quantity of inflammable air was produced, and there remained a lighter coloured substance, which may be called the *ashes of the metal*. Forty grains of this charcoal were reduced to eighteen by this treatment; and I collected about 200 ounce measures of air, which was very turbid in its first production, and burned with a lambent blue flame.

Having first produced this charcoal from *copper* (owing to my having accidentally found that the spirit of wine acted upon it) I proceeded to try the effect

effect of the same process upon other metals, beginning with *silver*. This metal I found to be affected very much as the copper had been; but though the matter with which the air was charged, was to appearance, equally black with that from the copper, and the vessels in which it was received, so minutely divided, acquired the same black coating, the larger masses of this charcoal of silver were much whiter than those of the copper.

Gold was not at all affected in this process, not being sensibly changed, or diminished in weight by it. At first, indeed, there issued a smoke of a darkish hue, the cause of which I could not discover, but it soon disappeared.

Finding this process to have so remarkable an affect upon copper, and none at all upon gold, I imagined that by this means, it might be possible to separate copper from gold, so that we should be in possession of a new and effectual mode of *assaying*. But I was disappointed in this expectation. I mixed ten grains of copper with an hundred of gold; but the copper was effectually protected by the gold from the action of the spirit of wine, and the whole mass came out of the same weight that it had before it was subjected to the experiment.

I was not able to procure much charcoal from *lead*. Using three ounce measures of spirit of wine, and near four ounces of lead, I got only a small quantity

quantity of a whitish powdery substance, though fifty eight grains of the lead were missing. But the inside of the glass tube, through which the inflammable air was transmitted, was very black, so that a great part of the lead was probably volatilized, and dispersed, and yet the heat that I used was not great.

Transmitting three ounce measures of spirit of wine over 360 grains of melted *tin*, it was not diminished quite four grains, and the black dust that was collected weighed twenty six grains. The air was very dark.

Lastly, I sent the vapour of two ounce measures of spirit of wine over 960 grains of *iron* shavings. The result was, that the air was charged with black particles, and the iron was diminished two grains in weight, but I was not able to collect any charcoal. The iron acquired by this means a dark blue colour.

PART

P A R T II.

EXPERIMENTS AND OBSERVATIONS RELATING TO
MERCURY.

SECTION I.

*Observations relating to the black Powder produced by
the Agitation of impure Quicksilver.*

BOERHAAVE found that quicksilver, by very long continued agitation, was in part converted into a black powder, which is often seen on the surface of it, and which, I believe, is generally deemed to be a partial calx of this metal, the mercury having parted with some portion of its phlogiston in this process. It is thought, however, that it is no great proportion of its phlogiston that it parts with in order to assume this new form of black powder, because it is not possible to expose it to any considerable degree of heat without completely reviving the whole of it. Even mere trituration has been observed to have the same effect. On this account,

account, some do not consider this process as a proper calcination, but suppose the mercury only to have assumed a *new form*, really containing all the phlogiston it was ever possessed of.

Notwithstanding this, I think it will appear from the result of my observations on the subject, that this black powder is really mercury *superphlogisticated*, having acquired more phlogiston, instead of having parted with any that had properly belonged to it; that various substances agitated together with mercury give it this overcharge of phlogiston, and to appearance resume it again. I also hope to shew in one view all the steps in the complete progress of mercury from this superphlogisticated state to its proper dephlogisticated state, the *precipitate per se*, in which it assumes four very different appearances. For the greater satisfaction of my readers, I shall, as I generally have done, relate my observations historically.

Having been under a necessity of making much use of quicksilver in my experiments relating to air, in order to separate and preserve those kinds that would have been absorbed by water, and being frequently obliged to remove my apparatus from the country to London, and from London to the country, I could not help being struck with a quantity of black powder, which I sometimes found on the surface of my quicksilver, when, at other

times, and, as far as I could judge, in the same circumstances, I found very little, or none at all. It was evident, however, that whatever was the cause of this appearance, the agitation of the carriage had contributed to it; for, except in those circumstances, I never found any of it. At one time I found, after removing my quicksilver, which was about twelve pounds, from London into the country, there was near a pound of this black powder on the surface of it. This I thought a great acquisition, as it was a quantity sufficient for a variety of experiments.

The first thing that occurred to me to do with it was to endeavour to expel air from it by means of heat. Accordingly, I put a quantity of it into a glass phial with a ground stopper and tube, and with the heat of a candle, I presently expelled from it a quantity of air, which being admitted to lime water made it very turbid, and was, in a great measure, absorbed; a proof that the air it had contained was in part fixed air, and the remainder was not so much diminished by nitrous air as common air would have been; so that no pure air came from this black powder, and consequently it differed essentially from the *precipitata per se*, which would have yielded no fixed air, but the purest dephlogisticated air only. I observed also, at the same time, that the powder at the bottom of the phial, which

which had been exposed to the greatest degree of heat, had become yellow. This was evidently something else than could have come from the quicksilver, but I did not at that time discover what it was. A good deal of the quicksilver was revived by the process.

Exposing another part of this black powder to a red sand heat in a glass vessel, I produced a greater quantity of fixed air. Also, part of the black powder became yellow, as before; and triturating the whole of it in the palm of my hand, it assumed a kind of dirty green colour, and about one half of it was pretty readily converted into quicksilver. Putting the remainder of this greenish powder into a thin glass vessel, and holding it over the flame of a candle, about one half of it became a perfectly yellow powder, and the rest was evaporated, and being in part collected, appeared to be pure quicksilver.

By this means I effected a complete separation of the quicksilver which had constituted the blackness of the powder, and had a perfectly distinct yellow substance behind, the nature of which an experienced chemist would have immediately distinguished; and I discovered it soon afterwards. I presently concluded that, notwithstanding this yellow substance seemed to be produced from the quicksilver, and had great specific gravity, it was not of

the nature of *precipitate per se*, because it had yielded fixed air. With another part of the black powder I found that the fixed air it yielded was several times the bulk of the powder, but I did not ascertain with exactness what the proportion was.

Being still ignorant of the constitution of this black powder, and being, consequently, unable with certainty to procure a quantity of it, I considered what other substances into which mercury entered had the same appearance, and among others I suspected that *Æthiops mineral*, which is a composition of mercury and sulphur, might perhaps be the same thing, and if so, it might be easily procured in any quantity for the purpose of future experiments. But I presently found that this substance, treated in the same manner in which I had treated my black mercurial powder, yielded no air at all.

Disappointed in this expectation, and being very desirous of procuring a quantity of this black powder, I took several quantities of this quicksilver, in the same state in which I had generally used it, and therefore, as I hoped, in the same state in which it had yielded the black powder before; and in order to treat it as nearly as possible in the same manner, I put it into such earthen pots as I had before made use of in conveying it from one place to another; and farther to promote a more minute division of its parts, I sometimes put sand, and other substances

on which I knew it could have no chemical action, into the pot along with it. I then put these pots into small boxes, and procured them to be fastened to post chaises, and other carriages, and had them brought to me again after they had undergone, at least, as much agitation as the former quicksilver had done in its passage from London to Wiltshire. But this produced no sensible effect; the quicksilver, as it appeared afterwards, being then too pure for that purpose.

At length it occurred to me that the quicksilver having been used for a great variety of purposes, and consequently having been exposed to a great variety of impregnations, it might have got some metallic ones, and particularly from lead or tin. I therefore dissolved a small quantity of lead in some mercury, and presently found that a very slight agitation covered it with black powder, and obscured all the inside of the vessel.

Being now in possession of what had been so long the object of my wishes, and being able to procure this black powder at pleasure, I was presently led by it to other observations both curious and useful.

In order to observe the nature and progress of this operation to more advantage, I filled a glass phial, of about ten ounces, one fourth part full of this mixture of mercury and lead; and inverting it

in a basin of the same, I agitated it with my hand, and presently found that the air within the phial was sensibly diminished, an evident proof that it was phlogisticated; and in about ten minutes the diminution amounted to one fifth of the whole, after which no agitation had any more effect upon it. Examining this air, I found, as I expected, that it extinguished a candle. Indeed it was completely phlogisticated; not being at all affected by nitrous air.

I was now fully satisfied that this was what I have called a proper *phlogistic process* with respect to air, similar to the calcination of metals by heat; the air being affected in the same manner, and that when mercury and lead were thus reduced to an amalgam, the simple exposure to air was sufficient to produce the calcination of one of them at least; and, as I then thought, of both, agreeably to the common opinion concerning the nature of the black powder of mercury.

I was abundantly confirmed in my supposition, by finding that when, instead of common air, I agitated this amalgam in fixed air, nitrous air, inflammable air, or in any kind of phlogisticated air, no black powder was produced, and those kinds of air remained unaltered. When, indeed, I agitated this amalgam in nitrous air, the surface of it presently assumed a blackish hue, but this soon nearly dis-

disappeared, and no farther agitation produced any sensible effect. But when, on the contrary, I made this agitation in dephlogisticated air, the black powder was generated exceedingly fast, and the air went on diminishing, till what remained was one fourth less than the whole.

It now occurred to me that, by means of this agitation, I might expel the whole of any quantity of lead, or other metals, from the mercury with which they might be mixed; and I soon found it to be an easy and excellent method, not at all inferior to distillation. As I have repeated this process many times, and always have recourse to it when my mercury has acquired any metallic mixture, I shall describe the manner in which I find it is most expeditiously done; though a novice in the process must not expect to succeed perfectly well at the first trial.

I take a glass phial with a ground stopper (such being generally pretty strong) containing ten or twelve ounces of water, and fill about one fourth of it with the foul quicksilver; then, putting in the stopper, I hold it inverted with both my hands, and shake it violently, generally striking the hand that supports it against my thigh. When I have given it twenty or thirty strokes in this manner, I take out the stopper, and blow into the phial with

a pair of bellows, which I do in order to change the air that has become in part phlogisticated, and knowing that the purer the air is the faster the process advances.

After a short time, if the mercury be very foul, the surface will not only become black, but a great quantity of the upper part of it will be, as it were, coagulated, so as to be easily separated from the rest. I therefore invert the phial, and covering the mouth of it with my finger, let out all the mercury that will flow easily, and put the black coagulated part into a cup by itself. This I press repeatedly with the end of my finger, till I make a complete separation of the running mercury from the black powder; and putting the powder by itself, I pour back the mercury to the rest of the mass out of which it was taken, in order to be agitated with it again.

This process I repeat till I find that no more black matter can be separated; and it is not a little remarkable, that the operator will be at no loss to know when the process is completed. For the same quantity of lead seems to come out of it in equal times of agitation, and consequently the whole becomes pure at once. Also, whereas, while the lead was in the mercury, it felt, as I may say, like soft clay, the moment the lead is separated from it, it

it begins to rattle as it is shaken, so that any person in the room may perceive when it has been agitated enough*.

That the mercury is made quite pure by this process I ascertained by distillation. For having distilled in a glass vessel a large quantity of quicksilver, in which both lead and tin had been purposely dissolved, and which had only been agitated in this manner afterwards, I found nothing more than a light whitish stain on the bottom of the retort.

When a quantity of the black powder is procured, it is very easy, by distillation, to separate the mercury from the calx, and I do not know a readier method of procuring the calx of lead, or tin, and perhaps the calx of other metals also. The quantity of black mercurial powder is very considerable in proportion to the lead or tin mixed with it; though it is not easy to ascertain this with exactness, because, in endeavouring to separate the powder from the running mercury a good deal of it is, by mere trituration, converted into running mercury;

* Pure mercury may also be distinguished from that which is very impure by this circumstance, *viz.* that a mixture of lead or tin, at least, very much diminishes its attraction of cohesion. For, when pure mercury is contained in a glass or earthen vessel, there will be a hollow space between the metal and the vessel; whereas if there be lead or tin in it, the whole surface, even to the place of contact with the vessel, will be perfectly level.

and

and I do not know but that, in time, the whole might be restored by this means, and the calx of lead, &c. be got quite pure. However, from the following experiments it will be seen what proportion they generally bear to each other, after a tolerably careful separation. It will be seen also, that when all the quicksilver that was converted into black powder is expelled from lead or tin by heat, there will remain more weight of the calx than there was of the metal; as might be expected. But as I applied more heat than was necessary to separate the quicksilver, a good deal of the air, and whatever else contributes to the additional weight of the calx, is, no doubt, expelled with it.

Having mixed one pennyweight of lead with about five pounds of quicksilver, I expelled it all by agitation, in the method described above; when, weighing the black powder, it was found to be one ounce, ten pennyweights, five grains, some particles of the running mercury being, however, still visible in it. When the quicksilver was expelled by heat, the calx of the lead appeared in the form of a brownish powder, and weighed one pennyweight, five grains.

Having mixed one pennyweight of tin with the above-mentioned quantity of quicksilver, and having expelled it again by agitation, the black powder, with some small globules of quicksilver mixed with

with it, weighed two ounces, one pennyweight, five grains, and the calx, which was a tolerably white powder, weighed one pennyweight, seven grains.

The separation of tin from quicksilver by agitation is not effected near so soon as lead. It requires at least four times the labour. It also requires proportionably more time to separate the black powder from the thick amalgam, in the manner described above.

Quicksilver is separated from lead or tin when the mass is agitated in *water*, as well as in air, but it seems to require more time. In this process it is also easily perceived when all the base metal is expelled; the phenomena of the agitation of this amalgam and of pure mercury in water being very remarkably different. It is even easy to perceive, by this means, in a moment, whether the quicksilver be pure or not. For if it be impure, the water becomes opaque the moment the agitation commences, which is by no means the case with pure quicksilver, especially if the water in which it is agitated has not been used for this purpose before. Also, the black matter suspended in the water in which pure quicksilver has been agitated is (except in a case that will be described hereafter) presently deposited; whereas the water in which the amalgam has been agitated does not become clear in several days. It may also be perceived how the quicksilver approaches

approaches towards purity, by this deposit being made more or less readily.

Also, the phenomena during the agitation in these two cases are strikingly different, though not easily described in words. More especially, the mixture of quicksilver with lead or tin does not seem to admit the water to mix with it, whereas pure quicksilver, by violent agitation, may be so thoroughly mixed with the water, that it will sometimes be several seconds after the agitation is discontinued, before it have entirely disengaged itself from the water; and in doing this it exhibits a very pleasing spectacle. By this means, as in the process without water, it may be perceived at once when the separation of the base metal, and the mercury, is completely effected.

Having a large quantity of water made very black with the agitation of a mixture of quicksilver and lead, I agitated a quantity of common air in it a long time, and let it stand several days; but the air was not sensibly injured by this means; so that though this water and the calcined amalgam suspended in it do contain phlogiston, it is not by this means imparted to the air.

I evaporated a pint of the distilled water in which quicksilver and tin had been agitated, and which had stood till it was quite transparent, when a white
sediment

sediment remained, but it did not weigh more than a few grains.

N. B. It may be worth while to observe that, in making this black powder, the phial in which the lead and mercury are shaken, grows very warm, as the coagulum of the two metals (from which the black powder is pressed) begins to form, and that in squeezing this coagulum in a cloth (which is the readiest method of separating the running mercury from the black powder) it suddenly becomes so hot, that I could sometimes hardly bear to handle it.

For the information of those who may wish to repeat these experiments, I would observe, that in using as much mercury as I can conveniently shake, in a phial containing about three pints of water, I have got, with four hundred concussions of the amalgam (blowing into the phial with a pair of bellows after every hundred concussions) near eight ounces of the black powder.

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SECTION II.

Of the Agitation of pure Mercury in Water.

AGITATION in pure water will convert the purest quicksilver into black powder, and much more speedily than it can be effected in air; but when this is produced in water, this state of the quicksilver is not permanent. But it will give my reader more satisfaction, if I describe the phenomena of this process just as they occurred to me.

I agitated a pound of pure quicksilver a few minutes in distilled water, when I observed that the water had become opaque, with particles of a black matter, so as to be impervious to the light. This process I repeated several hours, changing the water as it became black.

When any quantity of water had been once used for this purpose, the same effect was produced much sooner than it was with fresh water; so that, though the fresh water and this could not be distinguished by the eye, it was presently perceived which water had been used before.

After

After I had continued this process, which was in a ten ounce phial, with a ground glass stopper, about four or five hours, though with some interruption, I found that the quicksilver had lost two pennyweights of its weight. But, agitating it again little more than an hour, with the same water that I had used before, I found it had lost in all five pennyweights.

This process went on the best when I used three or four times the bulk of water with the quicksilver.

That the air contained in the phial together with the water had nothing to do in this business was evident, because the very same effect was produced when the phial was filled up with water only, so as to exclude all the air, and this is the manner in which I generally make this experiment.

This black matter diffused through the water becomes white running mercury when it is exposed to the open air only. No trituration, or operation of any kind, is requisite for this purpose.

The water in which this pure quicksilver had been agitated, acquired a peculiar smell and taste, not easy to be described. When a pint of it was evaporated to dryness, there remained a small quantity of matter, an account of which will be given hereafter. Common air agitated in this water was not sensibly diminished, and therefore I conclude not sensibly injured by it.

Spirit

SECTION II.

Of the Agitation of pure Mercury in Water.

AGITATION in pure water will convert the purest quicksilver into black powder, and much more speedily than it can be effected in air; but when this is produced in water, this state of the quicksilver is not permanent. But it will give my reader more satisfaction, if I describe the phenomena of this process just as they occurred to me.

I agitated a pound of pure quicksilver a few minutes in distilled water, when I observed that the water had become opaque, with particles of a black matter, so as to be impervious to the light. This process I repeated several hours, changing the water as it became black.

When any quantity of water had been once used for this purpose, the same effect was produced much sooner than it was with fresh water; so that, though the fresh water and this could not be distinguished by the eye, it was presently perceived which water had been used before.

After

After I had continued this process, which was in a ten ounce phial, with a ground glass stopper, about four or five hours, though with some interruption, I found that the quicksilver had lost two penny-weights of its weight. But, agitating it again little more than an hour, with the same water that I had used before, I found it had lost in all five penny-weights.

This process went on the best when I used three or four times the bulk of water with the quicksilver.

That the air contained in the phial together with the water had nothing to do in this business was evident, because the very same effect was produced when the phial was filled up with water only, so as to exclude all the air, and this is the manner in which I generally make this experiment.

This black matter diffused through the water becomes white running mercury when it is exposed to the open air only. No trituration, or operation of any kind, is requisite for this purpose.

The water in which this pure quicksilver had been agitated, acquired a peculiar smell and taste, not easy to be described. When a pint of it was evaporated to dryness, there remained a small quantity of matter, an account of which will be given hereafter. Common air agitated in this water was not sensibly diminished, and therefore I conclude not sensibly injured by it.

Spirit

Spirit of wine seems to answer this purpose as well as water, but not oil of turpentine. I exposed them, together with various other things, to continued agitation in a mill, for several months; but when the phial containing them was examined, neither the quicksilver, nor the oil of turpentine, was sensibly changed. Of these observations I shall give a separate account, at the close of this article.

Hitherto I was entirely ignorant of the real nature of the black powder into which mercury is converted by agitation in water, and rather took it for granted that it was a partial calcination of that metal; though I might have recollected, that no such thing as this black powder occurs in any part of the process of a proper calcination of mercury, in converting it into *precipitate per se*. Nor did I at length discover the real nature of it by any reasoning or conjecture *a priori*. But having constantly observed (what it was impossible not to observe) that whenever I spilled any of the water containing the black powder, the moment it was dry it appeared in the form of white running mercury, also that the glass funnel I made use of, in pouring this black water into the phial, was always found white, with small globules of running mercury, whenever I took it up, after an interruption in my experiments; I could not but conclude that this conversion of black into white mercury was effected by the air,

and therefore I determined to have this process performed in *confined air*, in order to judge how the air itself was affected by it.

Accordingly, having a considerable quantity of this black powder in a little water, enough to prevent its becoming running mercury, I poured some of it into a small retort, and evaporating all the water that was mixed with it, while the neck of the retort was plunged in water, and admitting as little air as possible (barely enough to prevent the retort from breaking by the rushing in of fresh water, after the bulk of the air had been expelled by the heat and vapour within the retort) I examined the inclosed air when the vessel was cold; and found it to be worse than common air. For one measure of this and one of nitrous air occupied the space of 1.31 measures; when one measure of the common external air and another of the same nitrous air occupied the space of 1.27 measures.

It was evident, however, that, in this experiment, the air could only be very partially affected by the change of the mercury; since a great deal must necessarily have been admitted after all the heat had been applied; and the newly admitted air must, of course, have diluted that which had been affected by the process. I therefore made the following more decisive experiment, which perfectly agreed with, and confirmed, the preceding.

I took a glass tube, about eighteen inches long, and half an inch wide, and pouring into it a quantity of the water and black powder of mercury, turned it every way till it had got a black coating in all places. I then inverted it, and placed it in a cup of water near the fire, but not so near as to convert the water within the tube into steam, and thereby expel too much of the air. In this situation I perceived, after some time, that the quicksilver was revived, all the tube to which the heat had reached having now got a white coating, and having the appearance of a looking glass. I then examined the air in the inside of the tube, and found it to be very sufficiently phlogisticated. For one measure of it and one of nitrous air, occupied the space of 1.66 measures, notwithstanding a considerable part of the tube had not been so much heated as to have all the mercury on it revived. I repeated this experiment in another tube, and with the same result, the air contained in it being as much phlogisticated as before. At this time the tube being exposed to too great a degree of heat, part of the mercurial coating was partially calcined.

After this it was impossible to entertain a doubt concerning the nature of this black powder. It was evidently *mercury super-phlogisticated*, or which had acquired more phlogiston than was necessary to its state of white running mercury. But it remained to be

be inquired whence the mercury could have received this phlogiston. That it might have been communicated from the spirit of wine in the experiment mentioned above was probable enough, because spirit of wine is known to contain phlogiston in abundance. But it has been a maxim with chemists, that water is incapable of forming any union with phlogiston; and that besides air, it is perhaps the only substance in nature that is incapable of it. However, as the whole course of my experiments has demonstrated the fallacy of the maxim with respect to air, so I think also it has already appeared from them, that neither does it hold good with respect to water.

Some may think that it is the *calx* of the mercury that the water seizes upon, leaving the phlogiston as an over charge upon part of the remainder. Which ever of these hypothesis is the true one, it is a fact, and certainly a very remarkable one, that, if the water be warm, though only about blood heat, no agitation of mercury in it will convert it into black powder. And also, if the water be ever so black with the powder, the mere heating of it, without any access of the external air, will make it transparent again; the blackness totally disappearing both from the water and the mercury. If the former hypothesis be admitted, *viz.* that the overcharge of phlogiston is communicated from the mercury to

the water, water must be of such a nature as to have a stronger affinity with phlogiston when hot than when cold, in which, though it be the reverse of most other substances, it has the same property, however, with *air*, which receives phlogiston from ignited bodies, when both the air and the ignited body must, of course, be equally hot.

Or, lastly, it may be supposed (and some observations that will be recited hereafter prove this is actually the case in some circumstances) that during the agitation one part of the mercury becomes de-phlogisticated while another part is super-phlogisticated, extraordinary as the fact will be thought.

I observed the effect of warm water in mercury in the following manner. Sitting pretty near the fire, when the weather was cold, I found that my agitation of the mercury had not so much effect as it had been used to have; and, reflecting upon the subject, it occurred to me, that possibly it might be the *warmth* of the water in the phial that obstructed it. To try this, I put the phial containing the water and the quicksilver into a pan of water, which I made to boil; after which I took it out of the pan, and holding it with a couple of handkerchiefs, agitated it with as much violence as I possibly could, but I found that I might do this as long as I pleased, without producing any thing like the black powder.

To

To complete this experiment on the effects of heat, as soon as the whole was cold, I shook the phial again, till the water was to appearance, almost as black as ink, and placed it in the water over the fire; observing that the phial was completely filled with the water and quicksilver, and that all air was excluded, only leaving the stopper rather loose, that the expansion of the water by the heat might not burst it. The effect was that, presently after the water in the pan began to boil, the water in the phial had recovered its transparency; and when I examined the quicksilver, there was no appearance of black powder upon it. The whole had been reconverted into white running mercury, and had united with the rest of the mass of mercury in the phial. Also, when it was cold, the blackness did not re-appear; but the mercury was, in appearance, in the very same state, as at the beginning of the process.

This fact being a very remarkable one, I repeated the experiment many times, and in a very great variety of ways, but always with the same result. When, indeed, I agitated the mercury in the same water a very long time (and I once did it on purpose a quarter of an hour, with little or no intermission, though in one minute I could make the water quite opaque with the same degree of agitation) I have found that it requires a longer continuance of heat to make it perfectly transparent, and a slight

blackness has remained on some parts of the surface of the quicksilver. But then this was quite trifling compared with the quantity of black matter that lay upon it before it was heated, so that by much the greatest part of the phlogiston must have been absorbed.

Also, when I have poured the quicksilver off, and heated the turbid water by itself, the blackness has never failed to disappear. But sometimes a few globules of quicksilver would remain at the bottom of the phial, some white, and others black; but though the latter were more numerous, they were probably only superficially black, and no agitation of the phial would ever give the water the turbid appearance that it had before; the globules, though dispersed through the water by the agitation, subsiding in a moment, and falling, like so many leaden shot, to the bottom of the phial; so that the black surface of these larger masses of quicksilver, as they may be called, was very small in proportion to the surface of the infinite number of black molecules which constituted the clouds of attenuated mercury that before had filled the whole phial, and made all the water in it opaque.

That other persons may more easily succeed in this experiment, I must inform them, that I have generally made use of a ten ounce phial, about a quarter of it filled with quicksilver, and the rest
with

with distilled water, shaking it as violently as I can, generally giving it ten or a dozen shakes in quick succession, in the manner described above; and then waiting till the water and quicksilver be separated from each other, which gives me a sufficient interval of time to rest from my labour.

The above-mentioned experiments may be made, with the same results, by substituting *spirit of wine* for water. After agitating some quicksilver in spirit of wine till it was very turbid, I placed the phial containing them in a pan of water, and presently after it had boiled all the blackness disappeared. Agitating it again, when it was hot, had no effect; and when it was cold the blackness did not return. The black powder thus procured became running mercury when it was dry, but it was not so bright as that which had been agitated in water. Letting a quantity of it remain six or seven hours upon a plate of glass, on the iron plate of a Bath stove, in which there was a pretty good fire, it lost its metallic lustre and consistence, and became a *white powdery substance*, which was completely dissolved in spirit of salt, and thereby appeared to be a perfect calx of mercury, though it was not brought to the state of *precipitate per se*.

Having now advanced another step in my investigation of the changes of mercury, in passing from the *super-phlogisticated* to the dephlogisticated

state; I went through the same process with the black powder procured by the agitation of mercury in water, covering with it the greatest part of the surface of a watch glass (which I find a very convenient thing for many small experiments) and placing it on the plate of the Bath stove, very near the fire, so that different parts of it might be exposed to different degrees of heat. The result of this experiment was very satisfactory and pleasing:

At first, as I have observed before, the black powder became running mercury; but presently after it adhered pretty firmly to the glass, and then, looking on the back side of it, I found it made the most perfect mirror imaginable, a better, I should think, than that which is made with mercury and tin. With a longer exposure to the same heat it lost its metallic lustre, and became a *white* powdery substance; and with more heat it assumed a *brown* colour. Yet a quantity of this brown matter, though, I doubt not, it was an approach to a proper *precipitate per se*, was not wholly dissolved in spirit of salt, so that the calcination had been imperfect.

It was pleasing to observe the mercury within so small a compass as that of a watch glass, in three of the states above-mentioned, *viz.* the white metallic state, the white calx, and the brown, or the dephlogisticated

phlogisticated state. On a larger plate of glass all the four states might have been exhibited in their natural order, the black powder, or the superphlogisticated state, preceding the rest. The order in which these different states of the mercury succeed each other is a proof of the hypothesis I have advanced on the subject.

In repeating these experiments I afterwards found some exceptions to this observation. Having a large quantity of this black matter in a glass phial, it happened to be broken by the freezing of the water with which it was mixed. I did not notice it at the time, but I afterwards found a considerable quantity of running mercury in the place where the bottle had stood, and besides that a thick cake of dry black matter, which however appeared to be nothing but mercury, as it was completely dissolved in spirit of nitre.

I must observe farther, that some of this black matter which I procured, by agitating mercury in water, has not always become perfect running mercury by the evaporation of the water with which it was mixed, but has sometimes left a black stain on the vessel in which it was contained. This, however, has disappeared by the affusion of spirit of nitre.

I shall now mention some other circumstances relating to the agitation of water in mercury, the
causes

causes of which I own I do not understand, and some of them seem to militate against the hypothesis advanced above. But this gives me no particular concern.

Indeed the greatest difficulty arises from the fact mentioned above, viz. that water which has been often used in this process has a much quicker and greater effect than water that is used the first time. This is more especially the case with water that has been distilled a long time. This certainly proves, that some change has been made in the water as well as in the quicksilver. But if the water communicates phlogiston to mercury, it might be expected that it would give it more readily at first than afterwards.

Also, if it be water that communicates the phlogiston to the mercury, it might be expected that water fresh distilled would have a greater effect, on account of the empyreuma that it is supposed to acquire by distillation, and which is known not to leave it of a considerable time. And, in general, I have found that water fresh distilled sooner becomes turbid in this process than water that has been long distilled, but not so soon as water that has been often used for this purpose. Also when I have re-distilled water that has been much used in these experiments, it has been as readily affected as before the second distillation; but with this difference,

ference, that the black powder has been much longer in subsiding than it had been before.

I have often found great differences in water in this respect. In general, if the water has been long distilled, and frequently used, the deposit will be completely made in a few minutes; whereas I have sometimes found that the water has not become clear (using the same mercury) in three or four days. And even when the blackness has disappeared, a white cloudiness will remain. I do not know how long.

I found some difference, but not so much as I had expected, between water distilled with a gentle heat without boiling, and water that was made to boil violently during the distillation, though both were distilled in glass vessels. They both became turbid pretty soon, and the quantity of black powder was nearly equal from both, in the same time; but that which had been hastily distilled deposited its sediment in about ten minutes, whereas the other had done it very imperfectly in an hour. I would not, however, be positive that a second experiment of this kind would have a similar result, as this circumstance may depend upon a cause not yet investigated.

There was something remarkable in the phenomena that occurred in using a quantity of water fresh distilled in a copper vessel, and a pewter worm,

worm, in the common way; but in which some *elder flowers* had been distilled about a year before; so that the water had a slight smell of it. But whether this circumstance has any thing to do with what I am going to describe, I cannot tell.

Agitating the quicksilver in this water, it presently became very turbid, but the sediment was not deposited in a week, or indeed completely, in a fortnight; and then the water retained a white cloudiness. But the most remarkable circumstance was that, in agitating the mercury in this water, the whole mass was presently divided into small globules, not larger than the smallest pins heads, and did not very readily unite again. Several times I have found that the mercury thus divided would choak up the mouth of the phial, which is about half an inch wide; so that, holding it perpendicularly, it would not run out at all in several seconds. It has even required shaking to get it all out. It has then exhibited a singular and beautiful appearance in the cup into which the phial was emptied, whereas the very same quicksilver agitated in other water, immediately before, and after, has been attended with no other than the common appearance. It was also remarkable, that this divided mass of mercury, after the most violent agitation in the water, fell instantly to the bottom, like a quantity of leaden shot; whereas, in general, as I have observed,

served, the mercury and water get intangled in such a manner, that they do not intirely separate in several seconds.

Imagining at first, that the power of re-union, in the divided mercury, might perhaps have been impaired by some effect of the small remains of the elder water, mixed with the fresh distilled water in which it was now agitated, I made trial of *mint water*, but without any such effect. A considerable time afterwards, however, I found other methods of producing the same effect, and even in a much more remarkable manner, though I am still at a loss to account for the *proximate cause* of the phenomenon.

Having a phial containing some water imperfectly impregnated with vitriolic acid air, and likewise a quantity of the quicksilver on which the impregnation had been made, I found that when they were agitated together the whole mass of quicksilver was divided into small globules, and that they did not perfectly re-unite after being at rest a day and night. But when the phial was *beated*, they united as readily as in common water. When it was cold again, the mercury was divided by agitation, and continued divided, but not quite so much as before.

This being an *acid* liquor, I made trial of other acids; and I found the same effect with *oil of vitriol*; but

but the division of the mercury into small globules did not continue very long, and when it was hot the effect was inconsiderable. But the most complete effect of this kind is produced by *vinegar*. A very little agitation of mercury in this acid divides it into the smallest globules; and they continue without any apparent disposition to re-unite even when very hot. While this divided mercury is in the vinegar the globules may be poured from one part of the phial to the other exactly like fine dry sand, and they exhibit a singular and beautiful appearance. All the vinegar must be evaporated by heat before these globules will unite.

Mercury agitated in spirit of salt, and also in a volatile alkaline liquor, was not attended with any remarkable appearance of this kind.

I have sometimes been much amused with another singular appearance. In agitating mercury in water, especially when fresh distilled (when there has not been a bubble of air in the phial) large balls of various sizes, some not less than half an inch in diameter, have not only rolled upon the surface of the mercury, after it had completely subsided, and continued there a considerable time, but have floated up and down in the water, like soap bubbles in the air. These bubbles must consist of water inclosed in a thin pellicle of mercury, for when they burst, nothing visible comes out of them, and the quantity

quantity of mercury about them is not enough to be perceived in its descent through the water afterwards.

I may also mention, as another pleasing phenomenon in these experiments, the viewing of a small quantity of the moistened black powder with the microscope. For in the instant that it becomes dry, the colour changes; and in so small a quantity the change is almost instantaneous, so that the black globules immediately become white, and beautifully polished ones.

In order to ascertain what change had taken place in the *water* in which mercury had been agitated, I distilled a quantity of it, and the result of the experiment is rather in favour of the water having seized upon the calx of the mercury, than of its having parted with any phlogiston to it.

After the distillation I found a considerable quantity of a yellowish residuum, which, when it was exposed to heat, on a plate of glass, became quite *black*, and with more heat was *brown*. Being exposed to the open air, it became very moist. Putting it, after this, into a glass tube, and exposing it to a red heat, a whitish matter sublimed from it, and coated the inside of the tube at some distance from it. This matter was not dissolved by spirit of salt; and therefore, though I think, from the
appear-

appearance of it, it was probably a calx of mercury, it must have been an imperfect one, containing a considerable proportion of phlogiston.

SECTION III.

Of the Effect of long continued Agitation on Quicksilver.

IN order to give quicksilver, in conjunction with various other substances, a much more, and a longer continued agitation, than I was able to give them by shaking the phials that contained them in my hand, I got a strong wooden box, and had a contrivance in a neighbouring mill to have it agitated whenever the mill was in motion, which I found was, at a medium, about twelve hours in twenty four. There was some difference in the circumstances of the quicksilver in all the vessels, and I shall give a brief account of what I observed with respect to them. The box was made up, and
sent

sent to the mill on the 9th of December, 1777, and the contents of it were examined on the 10th of May following.

No. I. An eight ounce phial with a ground stopper containing a pound of quicksilver, except five pennyweights which it had lost by frequent agitation in the same distilled water with which it was now shut up, the water being about four times the bulk of the quicksilver, marks being made upon the phial with a file, to denote the height of the water and of the quicksilver. When it was examined, the water appeared to have been diminished one seventh in its bulk, having possibly made its escape by the side of the stopper. The quicksilver had lost eighteen grains, which was probably the weight of the black powder that was formed in it; but, what I thought the most extraordinary circumstance, was that the bottom of the phial was tinged with a deep orange colour. Not willing to put other water, or other quicksilver into this phial, I made no other trial of the air, than by letting a small candle down into it, and I observed that, to all appearance, it burned very well.

No. II. A glass tube hermetically sealed, containing quicksilver and distilled water, which had been agitated one month before, in consequence of which a good deal of black powder had been form-

ed. This had received an increase of black powder, and part of the vessel was coated with the brown matter above-mentioned.

No. III. A three ounce phial with a ground stopper, containing quicksilver and water distilled in glass, about twice the bulk of the quicksilver. The surface of the mercury was well covered with black powder, and beside this, a good deal adhered to the bottom of the phial, and was also disposed in streaks almost surrounding it, in the middle of that part of the phial that had been occupied by the mercury. This black coating, viewed in a certain light, appeared of a dirty orange colour. A candle burned in the top of the phial.

No. IV. A three ounce phial with a ground stopper, about one fifth filled with quicksilver, without water. The quicksilver was well covered with black powder, and also a great part of the inside of the phial. A candle burned in it very well. Whether this quicksilver was perfectly pure I cannot absolutely say. If it had, I can hardly think there would have been so much black powder; and yet had it been very impure, the air within the phial would have been phlogisticated. If the quicksilver was pure, the agitation must have disposed one part of the quicksilver to part with its phlogiston, and another part of the same mass to have received it, which

which the circumstances in the other cases render probable; and if we admit this hypothesis, we shall be relieved from the supposition of the water, in the former experiments, communicating the phlogiston to the quicksilver, in order to the formation of the black powder.

No. V. A two ounce phial with a ground stopper, containing quicksilver and spirit of wine, the latter one and a half more in bulk than the former. The spirit was a little diminished in bulk, the mercury had more black powder upon it than there was in the phial containing quicksilver and water, and a compact body of this black powder covered one side of the phial; beginning at the surface of the spirit, and reaching to the top.

No. VI. A two ounce phial with a ground stopper, containing mercury and oil of turpentine, about one and a half as much as the bulk of the mercury. In this there was no sensible change.

I have observed that, in the phial in which quicksilver only had been agitated, and also in another which had contained both quicksilver and water, there was a quantity of brownish matter adhering to the glass. Had this matter been a calx of lead, mixed with the mercury, the air within the phial would certainly have been phlogisticated. Besides I am pretty sure that I had taken sufficient

care to have this mercury pure. I am therefore inclined to think, notwithstanding the peculiar manner in which it was produced, that it was the *precipitate per se*. The few observations that I did make upon it are all in favour of this supposition. When I exposed it to the heat of the fire, it became of a deep and proper orange colour, and when I exposed the phial that contained it to a great degree of heat, but not sufficient to melt the glass, the air within the phial was found afterwards to be rather better than common air, though not so much as that I could be absolutely certain the seeming difference might not have been owing to some accident in making the experiment.

But what I think the most nearly decisive in favour of this hypothesis is, that the phenomena attending the solution of this substance, and of the *precipitate per se*, in spirit of salt, are, in all the respects in which I compared them, the very same. This orange coloured matter in the phials was instantly dissolved by the spirit of salt, which, from being of a light straw colour, became colourless, like water; and when it was afterwards evaporated, it left a perfectly white substance behind. In all these particulars the solution of a small quantity of *precipitate per se* was attended with the same appearances. Also when a little of both the *residua* was
laid

laid on a thin plate of glass, and exposed to the heat of a candle, they were evaporated in a white smoke, exactly alike.

Admitting this substance to be a true *precipitate per se*, or a complete calx of mercury, we may perhaps explain the formation of the *black powder* produced by the agitation of mercury in water, by supposing that while one part of the mercury is super-phlogisticated, and becomes black, another part of the same mass is dephlogisticated or reduced to a calx, which is first white, but would in time assume an orange colour. And that the water dissolves a part of this calx, seems probable from the observation I made on the deposit made by it when it was evaporated.

SECTION IV.

Experiments proving the great Volatility of Quick-silver.

THAT mercury is volatile, even in the temperature of the atmosphere, when its surface is exposed to a *vacuum*, has been long evident from observations on the barometer, in some of which, exposed in the sun, a perfect distillation is perpetually going on; the invisible mercurial vapour always rising on the warmer side of the tube, and then forming into globules, and running down the opposite side, in the form of dense fluid mercury. But the experiments I have lately made seem to shew that this heavy substance is not less volatile when confined by vitriolic acid air, though pressed with the weight of the atmosphere, and that it is in some measure volatile, even when exposed to common air.

Presently after the discovery of vitriolic acid air, I observed that when the electric explosion was taken in it, which was done by confining it with quicksilver, in a glass syphon, so that the electric matter

matter was made to pass from the mercury in one leg of the syphon to the mercury in the other, the tube was presently covered with a black incrustation, and the longer the explosions were continued, the thicker this incrustation grew. I had not at that time, however, any suspicion that this black matter came from the quicksilver, but imagined that it was altogether formed from the vitriolic acid air. This I was then led to conclude from there being no such appearance when the electric spark was taken in marine acid air, though confined by mercury, in the very same manner.

Afterwards, observing the same black matter, though not procured with the same ease, or in so great a quantity, when the explosion was taken over mercury in common air, I could not help suspecting that this black matter came from the mercury; and this suspicion was confirmed by applying heat to it; for it was thereby converted into white fluid mercury. I thought, however, that it was produced by the electric explosion volatilizing the mercury, in consequence of falling directly upon it. For though the heat occasioned by such an explosion be confined to a small space, it is exceedingly intense.

That the explosion might not affect the fluid mercury, I next took it between two iron wires, half an inch above the surface of the mercury, in

the vitriolic acid air confined by it, and still had the black matter; which made it evident that the electric explosion did not produce the evaporation of the mercury, but found the mercurial vapour dispersed in the air. I also made the same experiment, and with a similar result, in common air. But in this case I could not produce the black matter, at least in any sensible quantity, at any considerable distance above the surface of the mercury; and in no respect were the appearances so striking, as when the explosions were taken in vitriolic acid air.

I took the electric explosion between iron wires at the distance of several inches above the surface of the mercury in this kind of air, and the blackness within the tube was produced just as much as it had been when the explosion was taken immediately upon the quicksilver itself; and on applying heat to the black matter formed in these circumstances, it presently became running mercury as before.

Having taken the electric explosion at various distances above the surface of the mercury by which the vitriolic acid air was confined, and always with the same success; I at last took it at the greatest distance that any glass tube I had by me would admit, which was about three feet above the surface of the mercury. But even in this case the black matter was, to all appearance, produced quite

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as readily, as when the explosions had been taken ever so near to the surface of the mercury; so that the mercurial vapour had completely pervaded this whole space of vitriolic acid air, and in a very short time; for I took the explosions presently after I had prepared the tube for the experiment.

But to be quite sure that this black matter did not proceed from the vitriolic acid air, I contrived to take the electric explosion in it when it was not confined by mercury. To do this, I completely saturated a quantity of water with this kind of air, confined in a glass tube, in the top of which I had cemented a piece of iron wire, which came within a proper distance of the extremity of another piece of wire, which reached to the bottom of the tube. The impregnated water was confined by mercury in the tube, and in the basin.

In these circumstances a small degree of heat made this water give out its air; so that all the upper part of the tube was filled with it, resting on the water only. Between these two wires I took large electric explosions a considerable time, but no black matter was produced. It is evident, therefore, that this black matter consists of mercury *super-phlogistified*; the phlogiston coming from the electric matter, when the explosions are taken in common air, but chiefly from the vitriolic acid air, which abounds with it, when they are taken in that air; and this accounts

for

for the appearances being so much more remarkable in this kind of air than in common air.

But though, in my experiments on mercury, recited before, mercury super-phlogistated by agitation in water, and assuming the form of a black powder, becomes white running mercury the moment that it becomes dry, this was not the case with the black matter in this process. However, when I moistened a little of it, and dried it again, I thought that part of its blackness disappeared, though not very sensibly.

Ethiops mineral is a composition of mercury and sulphur, and therefore resembles the black matter produced by these electric explosions in vitriolic acid air, and the vapour of mercury; the vitriolic acid air alone, as I have shewn, becoming sulphur in certain circumstances. I thought, therefore, that this black matter might be a real ethiops; but when I put a little of it upon hot iron, I did not perceive any blue flame to arise from it. If, therefore, this black matter be an ethiops mineral, the proportion of sulphur in it must be exceedingly small.

It still remained to be determined, whether this diffusion of mercurial vapour through the vitriolic acid air was occasioned by a proper *evaporation*, that is, by the repulsion of its particles, whereby it is made to assume an elastic form, and in that state to

mix with the air; or whether there be a *chemical union* formed by the mercury and this kind of air, and it therefore becomes *incorporated* with it. The following experiment seems to decide in favour of a proper evaporation.

I put a small globule of mercury into a narrow glass tube, communicating with the inside of the phial in which the oil of vitriol and copper for the production of vitriolic acid air were contained. But though I heated these materials, and continued the production of vitriolic acid air in these circumstances a long time, so that the globule of mercury was always kept exposed to a torrent of this kind of air, newly generated, I saw no prospect of its being at all diminished by it. I therefore conclude that vitriolic acid air does not properly take up, so as to combine with the mercury. However, it must be acknowledged to be difficult to account for the quantity of mercury contained in this black matter, in whatever manner it becomes diffused through the air, considering that this globule of mercury was not sensibly diminished. This, however, might possibly be owing to its being continually surrounded with a little moisture, from which I could not keep it free; owing perhaps to the oil of vitriol not being sufficiently concentrated, so that the watery part was thrown off by the heat.

I also

I also found much black powder whenever I decomposed dephlogisticated and inflammable air over mercury; and as it was no doubt of the same nature with that which was produced by the electric spark in vitriolic acid air, I shall subjoin the experiments relating to it.

In this process the inside of the vessel was always very black after each explosion, and when I poured in the mercury after the explosion, though there was nothing visible in the air within the vessel, there issued from the mouth of it a *dense vapour*. This was even the case, though I waited so much as two minutes after any explosion before I proceeded to put in more mercury in order to make another; which if the vapour had been *steam*, would have been time more than sufficient to permit it to condense into water. I even perceived this vapour when I had a quantity of water in the vessel, and the explosion was consequently made over it, as well as in contact with the sides of the vessel which were wetted with it; so that as this vapour had passed through the whole body of water when the vessel was inverted, it is probable that it must have consisted of something else than mere *water*. But I was never able to collect any quantity of it, though it must have been something produced by the union of the two kinds of air.

In

In order to collect a quantity of the matter that formed this vapour, I contrived the following apparatus. In a cork (*a*) Pl. VII. fig. 3, with which I could shut the orifice of the strong glass vessel (*b*) in which the explosions were made, I had two perforations. Through one of these (*c*) I poured the mercury, by means of a glass funnel; but into the other was introduced a glass tube, which, being bended, was inserted, by means of a cork, into a thin glass vessel (*d*) and went almost to the bottom of it. A small hole was also made in the cork, to permit the air to go out. Consequently, all the air that remained in the strong glass vessel, with whatever vapour it might contain, must, as I poured in the mercury, necessarily pass through the glass tube, and be diffused through the thin glass vessel; in which I imagined that all its contents, fluid or solid, must be deposited. However, though I repeated the experiment several times with this apparatus, making about twenty explosions in each, I could not find any deposit in the vessel, besides a small quantity of *water*; which, added to the water collected in the strong vessel, came far short of the weight of the air that was decomposed.

Being desirous of ascertaining, as perfectly as I could, the nature of that *black matter* which I procured by decomposing dephlogisticated and inflammable air, I frequently exploded great quantities
of

of the two kinds, and never failed to collect it. But I have not yet been able perfectly to satisfy myself with respect to the nature of it. From the following experiments it seems pretty certain, that *mercury* is necessary to its production, and I have never found it in vessels in which mercury had not been contained. I did not, however, find it to be acted upon by spirit of nitre so readily as that *black powder* which is got by agitating mercury in pure water (which can be nothing but mercury super-phlogisticated) but thought that it approached nearer to the action of that acid upon *foot*, the blackness of which is not discharged except by frequent affusions of that acid.

When there was much water in the vessel in which the explosions were made, there was no black matter produced, though the lower part of the vessel contained mercury; and though by repeated explosions in these circumstances, the water did become turbid, and deposit a black sediment, it was by no means so much as I always got when I only just moistened the inside of the vessel, by filling it with mercury immediately after there had been water in it. In this last case, all the inside of the vessel never failed to be made exceedingly black by every explosion; and repeating this process, I was able, by pouring in water, to collect a considerable quantity of black sediment. From this it should seem, that

that the inside of the vessel contained a subtle invisible vapour of mercury, which became black by the phlogiston from the decomposed inflammable air; and this black powder, as I have observed, is not much affected by spirit of nitre, but has very much the property of soot, in that respect, which is a circumstance that certainly deserves to be farther investigated.

At one time, having the inside of the strong glass tube made very black with these explosions, I let it remain a day or two exposed to the common air, when the blackness disappeared, leaving the inside of the vessel covered with small globules of white mercury. It seems, therefore, that part of the phlogiston of the inflammable air sometimes attaches itself to the vapour of mercury, diffused invisibly through the space within the vessel, and that it quits it to unite to the air of the atmosphere.

When I made a vessel containing green copper filings and a little mercury, and I introduced a quantity of new trainings (which is iron filings) into this vessel, exceedingly convenient for these and many other experiments) having previously made them together with the vessel the air, and the mercury, by which it was confined as they are, I could. **PART** Also, to prevent the air from entering any moisture, I received it immediately in

P A R T III.

EXPERIMENTS AND OBSERVATIONS RELATING TO
IRON.

SECTION I.

*On beating Iron in dephlogisticated Air, and afterwards
in inflammable Air.*

I BEGAN these experiments with a view to ascertain whether any *water* be produced when the air is made to disappear in them. Accordingly, into a glass vessel containing seven ounce measures of pretty pure dephlogisticated air, I introduced a quantity of iron turnings (which is iron in small thin pieces, exceedingly convenient for these and many other experiments) having previously made them, together with the vessel, the air, and the mercury, by which it was confined as dry as I possibly could. Also, to prevent the air from imbibing any moisture, I received it immediately in the

the vessel in which the experiment was made, from the process of procuring it from red precipitate; so that it had never been in contact with any water.

I then fired the iron, by means of a burning lens, and presently reduced the seven ounce measures of air to .65 of a measure; but I found no more water after this process than I imagined it had not been possible for me to exclude, as it bore no proportion to the air which had disappeared. Examining the residuum of the air, I found one fifth of it to be fixed air, and when I tried the purity of that which remained by the test of nitrous air, it did not appear that any phlogisticated air had been produced in the process; for though it was more impure than I suppose the air with which I began the experiment must have been, it was not more so than the phlogisticated air of the seven ounce measures, which had not been affected by the process, and which must have been contained in the residuum, would necessarily make it. In this case one measure of this residuum and two of nitrous air occupied the space of .32 of a measure.

In another experiment of this kind, ten ounce measures of dephlogisticated air were reduced to .8 of a measure, and by washing in lime water to .38.

In these experiments the fixed air must, I presume, have been formed by the union of the phlo-

giston from the iron and the dephlogisticated air in which it was ignited; but the quantity of it was small in proportion to the air which had disappeared, and at that time I had no suspicion that the iron, which had been melted, and gathered into round balls, could have imbibed any air; a melting heat having been sufficient, as I had imagined, to expel every thing that was capable of assuming the form of air from any substance whatever. I was therefore entirely at a loss about what must have become of the air.

Sensible, however, that such a quantity of air must have been imbibed by *something*, to which it must have given a very perceivable addition of weight, and seeing nothing else that could have imbibed it, it occurred to me to weigh the calx to which the iron had been reduced; and I presently found, that the dephlogisticated air* had actually been imbibed by the melted iron, in the same manner as inflammable air, in my former experiments, had been imbibed by the melted calces of metals, however improbable such an absorption might have appeared to me *a priori*. In the first instance, about twelve ounce measures of dephlogisticated air had disappeared, and the iron had gained six grains in

* It will appear, however, by subsequent experiments, that it was not dephlogisticated air that was imbibed by the iron, but only the water, which is by far the greatest part of it.

weight. Repeating the experiment very frequently, I always found, that other quantities of iron, treated in the same manner, gained similar additions of weight, which was always very nearly that of the air which had disappeared.

This calx of iron, I then concluded, was by no means what I had before taken it to be, viz. a *pure calx*, or *slag*, but either the calx, or the iron itself, saturated with pure air. This calciform substance I found, by various experiments, to be the same thing with the *scales* that fly from iron when it is made red hot, or the substance into which it runs in a very intense heat, in an open fire*.

Concluding from the preceding experiment, that iron sufficiently heated, was capable of saturating itself with pure air, extracted from the mass of the atmosphere, I then proceeded to melt it with the heat of a burning lens in the open air; and I presently found, that perfect iron was easily fused in this way, and continued in this fusion a certain time, exhibiting the appearance of *boiling* or *throwing out* air, whereas it was, on the contrary, *imbibing* air; and when it was saturated the fusion ceased, and the heat of my lens could not make any farther impression upon it. When this was the case, I always

* It is also the same thing with *finery cinder*, of which an account will be given hereafter.

found that it had gained weight in the proportion of $7\frac{1}{2}$ to 24, which is very nearly *one third* of its original weight. The same was the effect when I melted *steel* in the same circumstances, and also every kind of iron on which the experiment could be tried.

There was a peculiar circumstance attending the melting of *cast iron* with a burning lens, which made it impossible to ascertain the addition that was made to its weight, and at the same time afforded an amusing spectacle; for the moment that any quantity of it was melted, and gathered into a round ball, it began to disperse in a thousand directions, exhibiting the appearance of a most beautiful fire-work, some of the particles flying to the distance of half a yard from the place of fusion; and the whole was attended with a considerable hissing noise. Some of the largest pieces which had been dispersed in this manner I was able to collect, and having subjected them to the heat of the lens, they exhibited the same appearance as the larger mass from which they had been scattered.

When I melted this cast iron in the bottom of a deep glass receiver, in order to collect all the particles that were dispersed, they firmly adhered to the glass, melting it superficially, though without making it crack, so that it was still impossible to collect and weigh the particles. However, I generally found that, notwithstanding the copious dispersion,
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what remained after the experiment rather exceeded than fell short of the original weight of the iron. Sometimes a piece of common iron, and especially steel, would make a little hissing in the fusion, and a particle or two would fly off; but this was never considerable*.

Having now procured what I thought to be a new calx of iron, or a calx saturated with pure air, I endeavoured to revive it by making it imbibe inflammable air, in the same manner that I had before made iron, and various other metals, by melting them in a vessel containing inflammable air. In this I succeeded; but in the course of the experiment a new and very unexpected appearance occurred. I took a piece of iron which I had saturated with pure air, and putting it into a glass vessel containing inflammable air, confined by water, threw upon it the focus of the lens, and presently perceived the inflammable air to disappear, and without thinking of any thing escaping from the calx of iron (which had been subjected to a greater heat before) I imagined that I should have found the addition

* On being informed of the above-mentioned phenomena, Mr. Watt concluded, that the basis of the dephlogisticated air united to the phlogiston of the iron, and formed *water*, which was attracted by, and remained so firmly united to the calx of iron, as to resist the effects of heat to separate them. Neither he nor myself had, at that time, any idea of the quantity of water that is a constituent part of dephlogisticated air.

of the weight of air in the iron, and the result might be an iron different from the common sort. But I found, to my surprise, that the iron which had exhibited no new appearance in this mode of treatment, had lost weight, instead of gaining any. The piece of iron on which I made this first experiment, weighed eleven grains and a half, and seven ounce measures and a half of inflammable air had disappeared while the iron had lost two grains and a half.

Considering the quantity of inflammable air that had disappeared, viz. seven ounce measures and a half, and the dephlogisticated air which had been expelled from the iron, viz. two grains and a half, which is equal to about 4.1 ounce measures, I found that they were very nearly in the proper proportion to saturate each other, when decomposed by the electric spark, viz. two measures of inflammable air to one of dephlogisticated air. I therefore had now no doubt but that the two kinds of air had united, and had formed either *fixed air* or *water*; but which it was I could not tell, having had water in the receiver in which the experiment was made, and having neglected to examine the state of the air that remained, except in a general way, by which I found, that it was still, to appearance, as inflammable as ever.

With a view to determine whether *fixed air*, or *water*, would be the produce of this mode of combining

bining inflammable and dephlogisticated air, I repeated the experiment in a vessel in which the inflammable air was confined by mercury, and both the vessel and the mercury had been previously made as dry as possible. I had no sooner begun to heat the iron, or rather *slag*, in these circumstances, than I perceived the air to diminish, and at the same time the inside of the vessel to grow very cloudy, with particles of dew that covered almost the whole of it. These particles by degrees gathered into drops, and ran down the sides of the vessel in all places, except where it was heated by the sun beams; so that it then appeared to me very evident, that *water*, with or without fixed air, was the produce of the inflammable air, and the pure air let loose from the iron in this mode of operation; but I am now satisfied that the water I procured was the same that the iron had before imbibed. When I had examined the remaining air, it was as inflammable as ever, without containing any mixture of fixed air at all.

When I collected the water which was produced in this experiment, by means of a piece of filtering paper, carefully introduced to absorb it, I found it to be, as nearly as possible, of the same weight with that which had been lost by the iron; and also, in every experiment of this kind, in which I attended to this circumstance, I found that the quantity of inflammable air which had disappeared, was about

double to that of the dephlogisticated air set loose from the iron, supposing that weight to have been reduced into air. Thus at one time I made a piece of this slag imbibe five ounce measures and a half of inflammable air, while it lost as much as the weight of about three ounce measures of dephlogisticated air, and the water collected weighed two grains. Another time a piece of slag lost 1.5 grains, and the water produced was 1.7 grains; but perfect accuracy is not to be expected. I shall only mention one more experiment of this kind, in which six ounce measures and a half of inflammable air were reduced to .92 of an ounce measure, and the iron had lost two grains, equal in weight to 3.3 ounce measures of dephlogisticated air. In all the above-mentioned experiments, the inflammable air was that which is produced by the solution of iron in acids.

As before I had finished this course of experiments I had satisfied myself that inflammable air always contains a portion of water, and also, that when it has been some time confined by water, it imbibes more, so as to be increased in its specific gravity by that means, I repeated the experiment with inflammable air which had not been confined by water, but which was received in a vessel of dry mercury from the vessel in which it was generated; but I presently perceived that water was produced

in this case also, and to appearance as copiously as in the former experiment. Indeed, the quantity of water produced, which so greatly exceeded the weight of all the inflammable air, is sufficient to prove that it must have had some other source than any constituent part of that air, or the whole of it, together with the water contained in it, without taking into consideration the corresponding loss of weight in the iron.

I must here observe, that the iron slag which I had treated in this manner, and which had thereby lost the weight which it had acquired by melting in dephlogisticated air, became *perfect iron* as at first, and was then capable of being melted by the burning lens again; so that the same piece of iron would serve for these experiments as long as the operator should chuse. It was evident, therefore, that if the iron had lost its phlogiston in the preceding fusion, it had acquired it again from the inflammable air which it had absorbed; and I do not see how the experiment can be accounted for in any other way, which necessarily implies the reality of phlogiston as a constituent principle in bodies. This, at least, is the most natural way of accounting for the appearances.

Having had this success with the calx, or scales of *iron*, I tried the calx of *copper*, or those scales which fly from it when it is made red hot; and I found

found water produced in the inflammable air in the same manner as when I used the scales of iron in the same circumstances.

Iron, I found, acquired this additional weight by melting in an earthen retort, as well as in the open air by the sun-beams, if it were possible for it to attract air, or whatever else it is that is the immediate cause of its additional weight. Three ounces of common iron filings, exposed to a strong heat in an earthen retort, gained eleven penny-weights, or 264 grains, and yet was very far from having been completely fused. Having a glass tube communicating with the retort, in order to collect any air that the iron filings might give out, I found that when they were very hot, the water ascended within the tube; which shews that the iron was then in a state of absorbing, and not of giving out, any air.

Willing to try the effect of heating iron, and other substances, in all the different kinds of air, without any particular expectation, I found that iron melted more readily in *vitriolic acid* air than in dephlogisticated air, the air was diminished as rapidly, and the inside of the vessel was covered with a *black sooty matter*, which when exposed to heat, readily sublimed in the form of a white vapour, and left the glass quite clean. The iron, after the experiment, was quite brittle, and must,

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I presume, be the same thing with iron that is *sulphurated*; but I did not particularly examine it. Of seven ounce measures of vitriolic acid air, in one of these experiments, not more than three tenths of an ounce measure remained; of this two thirds was fixed air, and the residuum of this was inflammable. I had put three of such residuums together, in order to make the experiment with the greater certainty.

SECTION II.

Of the Quantity of inflammable Air yielded by Iron in its different States.

THE most important of all the *metals* is unquestionably *iron*, and it seems to be that which is as yet the most imperfectly understood, and in the manufacture of which there is the greatest room for farther improvements. But as all experimenters in air make use of it, at least for the purpose of procuring inflammable air (that being the cheapest process by which they can make it) it falls daily under their observation, and in conse-

quence of this it may be hoped, that we shall gradually gain a more perfect knowledge of it, and that this knowledge will lead to improvements in practice. I cannot promise much with respect to my own observations on the subject, but all *new facts* ought to be reported, and some that have occurred to me are of such a nature, as, in the hands of manufacturers in a large way, may perhaps be of some utility.

One of the greatest differences with respect to iron is between that which is called *cast iron* (which is the state in which it comes from the furnace) and that which is *malleable*. In the former state, though exceedingly hard, it is brittle; so that it can no more be brought into any other form without melting, than glass, but when melted, it still continues iron. In the malleable state, it is no longer capable of being melted again, and continuing to be iron, but it is flexible, and without melting, may be hammered into any shape that is wanted.

I had formed an idea, from what I had formerly read on the subject, that this great change in iron was made by nothing more than violent *hammering* while it was red hot; but when I saw the real process, I was soon convinced that this was a mistake. For in the finery furnace (or that furnace which is intended to refine the iron, and make it malleable) the cast iron is exposed to a strong blast of air, while

while it is red hot, or rather in a half melted state; and it is this blast of *air* that is the most necessary, if not the only necessary circumstance, in producing this change. Also, while the iron undergoes this operation, a quantity of liquid matter runs from it, and when it is cold acquires the form of a black cinder; which, from the name of the furnace out of which it flows, is called *finery cinder*. The nature of this substance, I think, I have investigated. When this finery cinder is separated from the common mass, what remains of the iron is actually malleable; but being in loose spongy masses requires the hammer to consolidate it, and to reduce it into a proper form for farther operations. Also, a considerable quantity of finery cinder remains in the interstices of the spongy masses, and the hammer is useful in separating it.

A farther remarkable change in the constitution of iron, is that by which it is converted into *steel*, which is made by *cementing* it with charcoal. It is then capable of being made elastic, and of receiving various degrees of hardness, according to the suddenness with which it is cooled after it has been made red hot. It is also then capable of being melted, without losing its metallic character.

At Birmingham, I find it is common to subject cast iron to this process of cementation; and then it is said to be *annealed*. This gives it a slight degree

gree of flexibility, so that it may be used for some kinds of nails. Having an opportunity of procuring these nails, both before and after annealing, I have made many experiments on them, and find that this process makes a remarkable difference in the chemical properties of the metal; and such as I should not have expected *a priori*.

Iron that is annealed, as well as that which is malleable, and also steel, is readily dissolved in diluted oil of vitriol; and leaves very little residuum; whereas the cast iron that has not been annealed, dissolves with great difficulty, and when it is dissolved, leaves a great quantity of black residuum. The solution itself is also attended with some appearances of a very striking nature, which I shall describe as they occurred to me.

From twenty four grains of cast iron, in the form of nails, I got, by oil of vitriol diluted with about three times as much water, in the course of some weeks (for without external heat less time is not sufficient) sixty nine ounce measures of inflammable air, exceedingly offensive to the smell. The nails still retained their form, and after repeatedly heating the mixture till it boiled, very little more air came from them, I examined them, and found them to be pretty soft, so that I could easily cut them with a knife. They had also the same highly offensive smell with the air that had come from them;

them; and being put into a jar of common air, they soon diminished, or phlogisticated, it.

When these nails were dry, they were brittle; and though white on the outside, they were black within; and after being hastily dried, they appeared to have gained fifteen grains. This experiment explains what I am told is commonly observed by those who put iron pipes into pits, in which there is water impregnated with vitriolic acid. For in time they become quite soft, or, as they call it, *rotten*, so that they can be cut with a knife. That a good deal of all the constituent parts of iron remains in this substance, was evident from the result of my melting a piece of it with a burning lens. For though it neither gained nor lost any weight, it spirted a little in the fusion, and threw out sparks, just like cast iron in the same experiment.

I was too hasty, however, in concluding that the process above-mentioned was over, as the next experiment will shew. Half an ounce of cast nails had been kept in oil of vitriol, diluted with three times as much water, about three weeks, and though the menstruum had been changed several times, and they had yielded 130 ounce measures of air, they retained their form. But I then perceived some specks of black matter floating in the liquor, and after a few days more, I found one third of the
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the phial in which the solution was made quite opaque with them. Nothing then remained in the form of a *nail*; and during this breaking-up of the nails, the air had come from them much more copiously than it had done for a considerable time before. For the whole quantity of air was now 190 ounce measures.

The air, however, comes pretty readily at the beginning of this process. For sixty ounce measures were procured in a very few days, whereas that quantity was not got in as many weeks afterwards, till the form of the nails was destroyed. The black powder that remained after this process had a very offensive smell; and when dried hastily, weighed eighteen grains. From another equal quantity of these nails this black powdery residuum weighed seventeen grains and a half, and this was the usual proportion of its weight to that of the iron.

What this *black matter* really is, how the process of annealing can so greatly lessen the quantity of it, and what connexion this circumstance has with the solubility of the iron in acids, are questions that well deserve consideration; and I think it very possible, that a little attention to the subject may lead to an easy solution of them, and a farther insight into the nature of iron.

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Another phenomenon in the solution of this cast iron being exceedingly remarkable, and with respect to myself, an *unique* (for I have never been able to repeat the observation) I shall mention the particulars exactly as they occurred to me, and as I noted them in my register at the time.

In the course of one winter, I had put half an ounce of cast iron nails into a mixture of oil of vitriol and water, and in the space of about two months, in the cold weather, they had yielded seventy two ounce measures of air; but the menstruum not having been changed, much copperas was concreted among the nails. In the following spring, when I began to give more attention to the process, I changed this liquor, and occasionally applied to it the heat of a candle, in order to accelerate the solution. A few days after this I observed in the liquor a great number of *transparent filaments*, a quarter, or half an inch in length, like the finest hairs, part of them adhering to the nails, at the ends, but the greatest quantity of these *hair-like crystals*, as they may be called, were detached from them, and floated in the liquor.

Taking some of them out of the menstruum, I found that they dissolved in fresh water, but not immediately. I then took them all out of the phial, and to the same nails put fresh oil of vitriol and water; and in a few days more of these crystals

appeared, but not so many as before. On heating the liquor while they were in it, they all disappeared; but after a few days they appeared again, and kept increasing very much. I made them disappear a second time in the same manner, and with the return of cold they appeared again. Having poured off this liquor, with the crystals in it, I heated that liquor, and made them disappear, but they reappeared in the same liquor when it was cold, and no more in the fresh liquor that was put to the nails. After this the nails broke up, and the air came very rapidly, as in the former process, and, in all, these nails yielded 163 ounce measures of air.

As these crystals were formed a little time before the nails broke up, I imagined that, whatever the substance was, it was that which had held them together, preventing their easy solution, and that it was attracted from the iron in the operation of annealing. I also thought, that by repeating this process, and watching it a few days before the breaking up of the nails, I could not fail to find the same appearance. But though I repeated the experiment, and with all the variations that I could think of, I was never gratified with a second sight of these beautiful crystals.

As these crystals were evidently of a saline nature, I concluded they were only martial vitriol, though

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in an unusual form. To ascertain this, I dissolved all the fragments I could collect of these crystals in water, and set it to evaporate. But instead of finding any green vitriol, there was only a black incrustation left on the evaporating vessel.

There has been some difference of opinion among chemists concerning the proportional quantity of air yielded by *iron* and *steel*. Having taken some pains to ascertain these facts, at the same time that I was dissolving iron and steel for other purposes, I shall here mention the result of my observations respecting the air that is yielded by all the kinds of iron that have fallen under my notice. I repeated the experiments several times, but the results often varied, without my being sensible of any change in the circumstances.

What quantity of air came from *cast iron* in the experiments mentioned above, I have noted in the account of them; but as I frequently repeated the experiment, especially with a view to find the hair-like crystals that I have mentioned, I shall here put down the different quantities of air, of which I also took an account in some of them, as well as of the residuums of black matter, which I observed at the same time. At one time a quarter of an ounce of cast iron gave ninety ounce measures of air, half an ounce gave 163 ounce measures, and two ounces gave 727 ounce measures, leaving a residuum of

black powder, which weighed ninety eight grains, and four ounces of the iron gave a residuum of 163 grains. Upon the whole, therefore, a quarter of an ounce of this iron may be said to give ninety ounce measures of inflammable air.

Of the *annealed cast nails*, a quarter of an ounce gave at one time 101 ounce measures of air, at another time 105½, at another 106, and the most of all 107. Half an ounce gave 212 ounce measures. Half an ounce of iron turnings, which is iron less perfectly annealed, gave ninety six ounce measures of air. It is evident, therefore, that the annealed cast iron gives considerably more inflammable air than that which had not been annealed; and this is easily accounted for, from the much greater quantity of residuum from the iron that has not been annealed, which residuum, it will be seen, contains much phlogiston.

A quarter of an ounce of *steel* gave ninety ounce measures and an half of air, 118½ grains gave ninety five ounce measures and an half of air, and the same weight of the very same iron from which that steel had been made gave ninety six ounce measures. Half an ounce of steel gave 207 ounce measures, and the black residuum, though greater than is found in the solution of iron, or cast iron annealed, was too small to be weighed. At other times, however, it has been much more considerable, though I have

have no note of the exact quantity that I found in any experiment. It is also observable, that in the solution of this half ounce of steel, in which there was so little residuum, the quantity of air was unusually great. And, I presume, that the differences in the quantities of air from given weights, both of steel, and different kinds of iron, will always be in some reciprocal proportion to the quantity of this residuum, which I have never found any solution of iron to be entirely without; and this will account for the different quantities of air that different persons have reported to have found both in iron and in steel. As a greater quantity of inflammable air was procured from annealed cast iron, than from malleable iron, I think it may be concluded, that steel (which is malleable iron annealed) will always give more air than iron, provided that, in the solution, it should yield no greater a quantity of black residuum. But in general there is much more of it found in the solution of steel than in that of iron.

This black powder which remains from the solution of iron or steel, and especially that from the cast iron not annealed, which is in such great quantity, well deserves to be thoroughly examined. Having made a few miscellaneous experiments upon it, I shall here give the result of them. Putting eighteen grains of it into spirit of salt, there remain-

ed eight grains undissolved. This was of a grey colour, but being sprinkled upon hot iron, it did not appear, by this test, to contain any *sulphur*.

I exposed a quantity of it to the heat of the burning lens in vacuo, and found the air that was expelled from it to be one ninth fixed air, and the rest inflammable, of the explosive kind. Ten grains of the powder gave nine ounce measures of air; but a good deal of this light powdery substance was unavoidably lost in conveying it through the water after the process.

This matter also appears to contain something of iron, though it is no longer soluble in oil of vitriol. For being exposed to the heat of the lens in open air, it was melted, and weighed just the same before and after fusion, which had also been the case with that cast iron which had been imperfectly dissolved in oil of vitriol, and retained its form.

From the air which this black powder gave, it may be concluded, that it contains much *plumbago*; but that which remains after the solution of malleable iron, or steel, is more nearly, or perhaps perfectly so. Nineteen grains of this (though it is possible that some of the other might have been accidentally mixed with it) was reduced to six grains, by being melted with the heat of the lens in the open air, and then became a glass, or slag; which was nearly the same result that I had from genuine *plumbago*,

plumbago, the greatest part of which is reduced into fixed and inflammable air.

The process of *cementation* is not, I am persuaded, sufficiently understood. It is the opinion of those who make steel, that the metal neither gains nor loses weight in the process. Those who anneal cast iron tell me, that it loses considerably in that process, which is similar to that of making steel, though the iron is taken in very different states. On the contrary, I found both malleable iron and cast iron to gain a little weight by cementation in my fires. Seventy two grains of iron wire gained three grains, and became of a dark black colour, and three ounces of cast iron nails gained six grains. But perhaps the heat that I used was too great for the purpose. For in consequence of this iron will attract water, and become in part *scales of iron*, which is always attended with an increase of weight; whereas in the same process with a long continued and moderate heat, it is very possible that some of the elements of that black matter, or plumbago, may go out of the iron, and join the charcoal, forming perhaps a sulphur, which may be sublimed, and dispersed in the process.

I have observed that I once expelled from some annealed cast iron nails such inflammable air as constituted fixed air, when it was decomposed with dephlogisticated air, which shews that it had im-

bibed some of that kind of inflammable air which is peculiar to charcoal. But when I dissolved that kind of iron in vitriolic acid, the air was the very same with that which came from malleable iron, no fixed air being produced in the decomposition of it.

SECTION III.

Experiments on Finery Cinder.

THE most useful of my observations on the subject of *iron* relates to the nature of the *finery cinder*, the substance which, I have observed, runs in a liquid form from cast iron in the process of converting it into malleable iron. This has generally been considered a thing of no value; and is commonly thrown away as such, though my brother-in-law, Mr. Wilkinson, has, with advantage, made use of a certain proportion of it in the smelting of iron. I flatter myself, however, that by a due attention to some observations which I shall here relate, this substance may hereafter be employed

to more advantage. But before I mention this use of it, in reducing it to iron, I shall recite some experiments which I made with a view to ascertain the nature of it.

The first thing that occurred to me, was to attempt the solution of it in the several mineral acids, which I did with the following results. Putting sixty grains of it, finely pulverized, into each of the three acids, and leaving them two or three days to digest; of that which had been in the vitriolic, and also of that which had been in the marine acid, there remained fifteen grains undissolved; but of that which had been exposed to the nitrous acid, forty grains. Another time, I observed that of seventy-four grains exposed to the spirit of salt, thirty remained undissolved.

This substance, as it comes from the common furnace, is seldom without some portion of iron, as appeared by melting it in the sun, when twenty-four grains of it gained one grain in weight. It also gained a little weight when I exposed it to the action of steam in an hot earthen tube.

Finery cinder, when hot, readily unites with most kinds of earth, greatly promoting their fusion, in the same manner as the *fluor*, which has its name from that circumstance. Attempting to get air from the finery cinder in an earthen retort, seventy ounces of it yielded forty ounce measures of air; but

but as the retort was dissolved, and most of the contents run out, nothing certain could be pronounced concerning the origin of this air, of the last portion of which three fourths were fixed air. The standard of the residuum was 1.67. In one part of it a candle was extinguished, but another portion was slightly inflammable.

The circumstance that first gave me an insight into the real nature of this finery cinder, was my having found a piece of malleable iron become throughout a perfect finery cinder, in consequence of being left in the open fire of my furnace, when the heat had been very great. The appearance of it alone satisfied me, that it was the same thing; and comparing it in a variety of processes with real finery cinder, I found that they did not differ in any respect. They appeared also to be the same substance with the *scales* which fall off from iron when it is heated in an open fire, with that which malleable iron is brought to by being melted in the open air, or in dephlogisticated air, and also by being exposed to the action of steam when it is red hot. In all the different processes recited before, it was perfectly indifferent which of these substances I made use of, and therefore I omit to mention the particulars.

Finery cinder is therefore to be considered as iron increased about one third in weight by imbibing water,

water, though with the loss of its own phlogiston. Nothing, therefore, is wanting to bring it to the state of iron again, but the expulsion of the water, with which it is saturated, and giving it the phlogiston which it has lost: and this is readily done by heating it in close vessels, in contact with any thing that contains phlogiston. This is effected in the most complete manner, by heating it with a burning lens in inflammable air. But it is likewise done cementation with charcoal, with coak from pit-coal, or with raw coal. In all these processes, the finery cinder loses about one third of its weight, and is then perfectly soluble in acids, and attracted by the magnet. Consequently, it is *perfect iron*. But whether it can be made useful iron, and in a manner so cheap as to make it worth the while of the manufacturer of iron, to establish any work for the purpose, is not for me to say. I would only observe, that iron thus made from finery cinder in my retorts, is not in the state of simple *cast iron*, but of that which is annealed, and likewise partially malleable.

B O O K XI.

MISCELLANEOUS EXPERIMENTS AND
OBSERVATIONS.

SECTION I.

Of the Electric Spark, in different Liquids.

ONE of my first experiments on the subject of air was that by which I procured inflammable air, by taking the electric spark in different kinds of oil. In the acids no spark can be taken, on account of their being such excellent conductors of electricity. However, a small bubble of air may be left confined by any acid, and in this a spark may be taken, which will affect the contiguous fluid. This I have done, and some of the results are sufficiently remarkable; but similar to the effects of transmitting the liquors in vapour, through a red hot earthen tube, in which case the nitrous and vitriolic acids yield dephlogisticated air.

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In order to take the electric spark in *nitrous acid*, I admitted a small bubble of common air into a glass syphon, previously filled with that acid, placing each leg of the syphon in a different vessel, containing some of the same acid, and I transmitted the spark, or shock, through a gold wire, on which the acid has no action. In these circumstances every spark made a considerable addition to the quantity of air, and it appeared to be the purest dephlogisticated air.

With the *vitriolic acid* I had a similar result, but much more time was required to produce the effect. After taking the spark two hours in a bubble of air confined by this acid, in a syphon, the greatest part of which was filled with mercury, and the legs of it standing in basons of mercury, the bubble was not more than doubled in quantity; but the addition that was made to it appeared to be dephlogisticated air, the standard of it being, with equal quantities of nitrous air, &c. It may be worth noticing, that though the oil of vitriol, used in this experiment, was of a light purple colour (which might be the reason why the air procured from it was not both more in quantity, and better in quality) the part next to the air became perfectly colourless for the space of about a quarter of an inch.

When I repeated this experiment with common *spirit of salt*, the bubble of air was diminished one fourth;

fourth; and though the operation was continued a long time, it never increased afterwards, and was simply phlogisticated air. When I used dephlogisticated marine acid, the bubble of air was diminished near one half, but what remained was still phlogisticated air.

With *phosphoric acid*, the air was first diminished one quarter, and then increased. This air I could perceive not to be affected by nitrous air; and by the redness of the electric spark in it, I have no doubt of its being inflammable air. But the quantity was too small to ascertain it in any other manner. This acid is always said to contain phlogiston, and by this means it seems to be converted into inflammable air.

I had a similar result when I took the electric spark in air confined by *phlogisticated alkali*. It was first diminished one fourth, and then increased to its original bulk; but the increase was very slow. When I examined the air, I could only observe that it was not affected by nitrous air. The addition must, I think, have been inflammable air, though the quantity was so small, that the spark had no sensible redness in it.

I shall conclude the account of these experiments with mentioning one of small consequence, relating to the conducting power of substances. Having boiled some linseed oil, I put into it a quantity of

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pounded glass. When it was cold it was a solid black mass, and conducted electricity perfectly well through its whole substance, and was not, as I had expected, that kind of substance which conducts electricity in a visible manner, such as I have described in my observations on charcoal, and which I had often found to incrust pieces of glass that had been made red hot in vegetable oils. I then melted the whole mass; but though it remained black throughout, and had been covered from all access of the external air, it had no more conducting power than any other kind of glass.

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SECTION II.

Of the conducting Power of certain Substances.

THAT *conducting power*, with respect to electricity, depends upon the variable state of substances, is evident from a variety of experiments. Thus glass, which when cold is a perfect non-conductor, is a complete conductor in a great degree of heat. So also, by a contrary process, *ice*, which when formed in a moderate degree of cold is a conductor, very much like water, becomes, as Mr. Achard has discovered, a non-conductor in a greater degree of cold. And I had found that though *dry wood*, and even *charcoal*, made with the least possible degree of heat, is a non-conductor, yet when it has been exposed to *more heat*, it is the most perfect of all conductors, not exceeded even by the most perfect metals themselves. I have now observed what, indeed, was not perhaps very difficult to be conjectured, that *water*, and even *quicksilver*, in the state of vapour, are no conductors of electricity.

Water had been often tried in that kind of vapour which is just condensing, in the open air; but

then it is, in fact, no other than water in very small drops; whereas, to try it in the proper form of *steam*, it must be examined in a degree of heat, in which it is incapable of condensing into water. This I did in the following manner :

I filled a glass syphon with water, having previously put iron wires into each of its legs, as is represented Pl.V. fig. 5. and then inverting it, placing each leg in a separate basin of water, or quicksilver. After this I exposed the upper part of the syphon to a degree of heat capable of converting water into steam. Then, bringing a charged phial, and making the syphon part of the circuit, made the explosion pass from one wire to the other, in the bend of the syphon. In this case the spark never failed to be as visible, as it would have been in the air. The only difference was, that in this case the spark was reddish, as it is when taken in inflammable air. I could perceive no difference whether the heat was greater or less, even in the very point of condensing into water. It is possible, however, that there might be some real difference, though not discernible in this method of examining it.

In the very same manner, I made the experiment in the vapour of *quicksilver*, having filled the syphon with quicksilver, and placing the legs of it in basins of the same. In this case, also, the electric

explosion was red; but at one time it was quite vivid. I repeated the experiments many times, both with water and with quicksilver.

From these experiments, compared with similar ones that I have made in all the different kinds of air, I think it may be concluded universally, that all substances, in this expanded state of air, or vapour, are non-conductors of electricity.

There is something exceedingly difficult to account for in the circumstances in which glass jars sometimes break spontaneously with electrical explosions. In general the thinner the glass is, the more liable it is to a fracture in this case. I observed, however, in my *history of electricity*, a case in which a very thick glass jar broke, in a very remarkable manner, by a spontaneous discharge; and I have lately observed another hardly less remarkable.

I filled a glass tube, about three feet long and one inch and a quarter wide, the glass itself being not less than one eighth of an inch thick, half full of quicksilver; and putting a loose coating of tinfoil on the outside, and beginning to charge it, by means of an iron wire connected with the prime conductor, it presently broke by a spontaneous discharge, exactly at the bottom. A large piece of the glass came out, and the quicksilver flowed out

at the hole. Examining it more particularly, it appeared that there were a great number of small independent fractures, but all very near together; and through one of them only the charge had made its way, pulverizing the glass, as usual.

I then charged a long tube of *bottle glass* in the same manner; but this also burst as soon, and also exactly at the bottom, though not in so many places. I meant to have charged these tubes, and to have sealed them hermetically, after I had poured out the quicksilver, in order to observe how long so thick a glass would retain the charge, in pursuance of Mr. Canton's first observation of this kind.

SECTION III.

Observations on Substances exposed to a long continued Heat.

MY experiments on exposing substances to a long continued heat were begun, principally with a view to ascertain the conversion of water into earth, of which we have many credible accounts, and of which that excellent chemist, Mr. Woulfe, entertains no doubt.

For this purpose I provided glass tubes, about an inch in diameter, and three feet long, and also others made like what the workmen call *proofs*, growing narrower to the top, some two inches wide at the bottom, and others less than an inch. Indeed, I used glass tubes of a great variety of forms and sizes, and when I had put in the water, or other fluid, I closed them hermetically, and placed them in a sand furnace pretty equally heated. But, in general, before I placed them there, I exposed the end containing the fluid near a common fire, for a few hours; both to observe whether there would be any
immediate

immediate change, and also to try what degree of heat the tube, thus charged, would bear.

The result of many of the experiments made in this manner have been recited, and were sufficiently remarkable, and others, that do not deserve to be passed over, will be noted in the course of this section. But with respect to *water*, which was my first and principal object, all my experiments intirely failed.

In order to avoid expence, I used a greater degree of heat than had been used before for this purpose; hoping, by this means, to gain my end in less time. Whereas I believe Mr. Woulfe's opinion is quite right, *viz.* that the heat should be very moderate, and long continued. Mine was considerably above a boiling heat in the open air, generally such as to keep the water boiling in this confined state, my vessels being strong in proportion. I went upon the idea, that the change of consistence in water was brought about by extending the bounds of the repulsion of its particles, and at the same time preventing their actually receding from each other, till the spheres of attraction within those of repulsion should reach them. The hypothesis may still be not much amiss, though I did not properly act upon it.

Be this as it will, a trial of six months had no effect of the kind that I hoped for.

The particular appearances that I observed would be too tedious to relate, and were not of much importance. I shall, therefore, only observe in general, that I was deceived at the beginning of the process, by finding that the whole mass of water, which was generally an ounce, would become exactly like milk, and sometimes the whole tube would have got a complete white coating in the course of a day or two. This I then hoped was, in part, a change in the water itself, though I had no doubt but that, in part, it might be owing to the corrosion of the glass by the heated vapour. In the end it appeared to have been nothing at all else.

When the heat was a little more moderate, the first appearance was a white pellicle on the surface of the water, and sometimes in the middle of the water only, not extending to the sides; which deceived me the more into an opinion that this earthy pellicle might come from the water itself. In time there was such an accumulation of this matter, that it clouded the whole mass of the water, and sunk to the bottom, in the form of white flakes, or a powdery substance. When the tubes were opened, all the sides were found corroded, the polish being entirely taken off where the heat had been greatest, especially near the surface of the water.

The force of the vapour of water in thus corroding glass is, however, not a little remarkable.

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In time it would have worked its way through any thickness of it. And, indeed, I should observe, that the same is the case with iron. For before I began these experiments, I had made a few random trials of what might be done with water *in a short time* by a very great degree of heat, in a confined state, by putting the water into gun-barrels, then getting them closed by welding, and after that putting one end of them into a hot fire. Sometimes the water would continue thus a whole day or more; but at length, though the gun-barrels were the thickest that I could meet with, and one of them was the breech of a musket-barrel, and I believe perfectly sound, it wore its way through. None of the barrels were properly *burst*, but all of them were much corroded, and made exceedingly thin in particular places; and when they were opened a great quantity of rust was found in the insides of them*.

Besides trying the effect of this process on pure distilled water, I made trial of water impregnated with all the different kinds of air with which I am acquainted; and in other tubes the air confined along with the water was of all the different kinds; but the appearances in them all were nearly the

* I since recollect that I formerly had a copper æolipyle, not less than the thickness of a half crown, which, after being used a good deal, burst, and was found to be as thin as paper.

same, excepting such as have been, or will be particularly described. The common air, in all these tubes, in which the water had been kept so hot, did not appear to have been changed either for the better or the worse. Sometimes when I softened a part of a tube with a blow pipe, the inclosed air would press the glass a little outwards, and sometimes the external air would press it a little inwards, but it was with no great force; and whenever I opened the tubes under water, and examined the air, it did not appear to have been altered in its quality, with respect to its diminution by nitrous air.

It is known, that, in general, a menstruum will hold more of a *solvend* when it is hot, than when it is cold; but these experiments in a continued heat afford several remarkable examples of the contrary. The first thing I observed of this kind was with respect to *lime water*. For having confined a quantity of it in one of my largest tubes, I found that, in six days, and how much less time might have sufficed I cannot tell, all the lime was deposited. At least there seemed to be enough at the bottom of the water from which it was separated, to have saturated the whole of it.

Also *iron* dissolved in water impregnated with fixed air was seemingly all precipitated, in consequence of being exposed in the same manner to the heat;
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and when it was cold, it was not re-dissolved. For though this menstruum will dissolve iron, it will not dissolve the calx of iron.

I had been informed by Mr. Bewly, that lime water would discharge the colour of Prussian blue. A quantity of lime water, thus impregnated with the colouring matter in Prussian blue, I put into one of my glass tubes on the 11th of August, and on the 23d, from being quite colourless, it was become of a greenish colour, with many opaque particles in it. On the 9th of September following it was quite transparent, with a large white sediment, in which it resembled the tubes that had only water in them. This sediment, therefore, might perhaps come from the corrosion of the glass. On the 30th of September the liquor was quite cloudy, had a considerable precipitate, and a thick whitish incrustation covered all the surface of it. Lastly, on the 19th of January, 1778, it had something of a milky appearance, but was nearly transparent, and had deposited a quantity of flaky matter.

Having the solution of *mercury*, and also of *copper* in spirit of nitre at hand, proper tubes to spare, and room enough for them in my hot sand, I placed about an ounce measure of each of them in the furnace on the 9th of September, and on the 30th of the same month I found the solution of mercury quite colourless as at first; but I suppose the

greatest part of the mercury was precipitated in one beautiful compact yellow mass. The precipitate of the copper was also collected into one mass, quite blue, as the liquor itself continued to be; so that the whole of the copper had not been precipitated.

When I took these tubes from the sand heat for a few days, the greatest part of the precipitated mass was re-dissolved; but when they were replaced in the sand heat they appeared again as at first; and so they were found on the 19th of January, 1778; when an end was put to the process.

On the subject of the nitrous acid I shall observe, that water saturated with nitre, which had been placed in the sand furnace on the 3d of September, in a long and slender glass tube was transparent on the 30th of the same month; but the tube itself, from the surface of the liquor to half an inch below it, and likewise in different places quite to the top of the tube, was covered with a white incrustation, a little inclined to blue.

Caustic alkali impregnated with nitrous vapour had cracked the tube in which it had been confined, and escaped; but the tube was found covered with a white incrustation, from two inches above the surface of the liquor quite to the bottom of the tube. The crack itself was very remarkable, consisting, in reality, of many different cracks, and those disposed

posed very irregularly, quite round the glass, near the surface of the liquor. I have sometimes seen glass cracked in the same manner by electrical explosions.

The most remarkable thing that I have observed, with respect to metallic solutions, relates to a solution of *gold in aqua regia*, made by the impregnation of the marine acid with nitrous vapour, which I have observed to be a more powerful menstruum for gold than the common aqua regia. A small quantity of this solution I had put into a very thick glass tube about nine inches long, and I placed it in the sand furnace on the 11th of August, and on the 23d of the same month, I found much of the gold precipitated, and adhering to the sides of the glass in the form of slender crystals, very beautiful. On the 30th of September, I observed no difference in the crystals, but found some gold precipitated in irregular masses, of a darkish colour, quite distinct from the crystals; and thus it remained till the 19th of January following, when I discontinued the process. Both the crystals and the gold still continue not re-dissolved.

I shall now just mention my observations on some other substances exposed to the same heat, though they have nothing in them that will be thought of any consequence; except that it may be proper to

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be known that the experiments have been made, and that no remarkable appearance followed.

Spirit of wine in large tubes underwent no alteration, nor did it affect the glass in the least; but another quantity confined in a short tube, and exposed to much more heat, appeared on the 30th of September (having been placed in the furnace on the 11th of the same month) to have given to the inside of the tube, and especially to the middle part of it, a thin bluish coating, a little inclining to white. Thus it continued to the last, except that the coating became more white, and had very nearly, if not wholly, lost its bluish cast.

Ether had also been confined in a short and strong tube on the 11th of August, and it continued colourless; but on the 30th of September several parts of the inside of the tube had a whitish incrustation, the glass being probably affected. Thus it continued till the end of the process, in January following, except that I then observed the whitish incrustation about an inch above the surface of the ether, at both ends of the tube; owing, I suppose, to my having, at different times, placed both the ends downwards.

With ether I also made another experiment somewhat similar to the above. Having filled a glass tube with it, I poured it out again, and immediately sealed

sealed it hermetically; then holding it in the flame of a candle, I observed a whitish cloud formed in the inside, and when the whole tube was exposed to the heat of the fire, and was made nearly red hot, part of it became whitish; but the air within the tube was not sensibly changed. I made the experiment in imitation of that with the inflammable air, which made the tube become black; thinking that, if the phlogistic matter had produced that effect in this case, it might do the same in another.

Olive oil exposed to a very great degree of heat, in a short and strong tube, was not changed. But in a large tube (owing, I imagine, to some bit of straw, or some other substance containing phlogiston, which, unperceived by me, might be in the tube) the oil became, in the interval between the 11th and the 23d of August, quite black, and of the consistence of treacle, with a smell strongly empyreumatic and offensive. I put part of this matter into another tube, but it was broke by some accident, and what remained of the matter was as hard as a coal, and quite black.

Oil of turpentine, which was quite colourless, became, in the same time, quite yellow, like dark coloured olive oil. It had also some opake particles in it. The glass being softened, it was pressed inwards. On the 9th of September the colour of the general mass was the same, but there were several

ral small lumps at the bottom, exactly like rosin to appearance. They did not adhere to the glass, but rolled about at the bottom, being heavier than the fluid mass. In a short glass tube, also, oil of turpentine was a little yellow.

Distilled vinegar suffered no change by being exposed in a long glass tube to a common fire for about an hour. But common vinegar, in the sand furnace, was turned almost black in the course of three weeks. But I ascribe this effect to some phlogistic matter contained in it. After the process, the taste of it was evidently less acid, like vapid vinegar, and the air within the tube was injured; one measure of this and one of nitrous air occupying the space of 1.4 measures.

After this I placed distilled vinegar in the sand furnace; and this, in the interval between the 9th and the 30th of September, had made a deposit of some black matter, and the tube was coated with it quite round, at the surface of the liquor. Also, in a short tube, the same vinegar was a little opaque, and there was some black matter on one side of the tube, half an inch above the surface of the fluid. In this state these tubes continued to the last, when they had deposited a brownish sediment.

Having exposed a small quantity of water impregnated with *fluor acid air*, quite transparent, in a glass tube hermetically sealed, to the heat of a
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common fire, I observed that, presently after it began to boil, it became of a dull blue colour, and a whitish vapour rose from it, as high as the middle of the tube. Afterwards, the heat increasing, it became transparent again, without depositing any thing, even when cold.

Repeating the same process, I observed the same cloudiness come on after boiling about an hour, but after continuing to boil two or three hours, it disappeared again. This cloudiness is exactly like the appearance of this impregnated water when some of the fluor crust is mixed with it. This experiment, therefore, proves that this liquor, in its most transparent state, contains a quantity of fluor crust dissolved in it, as I have observed before, in my attempts to account for its not freezing, when water impregnated with vitriolic acid air will freeze.

The effect of a continued heat on the *volatile alkaline liquor* was much the same with that on the acid impregnations. I exposed, in a glass tube, four feet long, and one third of an inch wide, a quantity filling about the space of an inch of caustic sal-ammoniac bought at the apothecaries; and in less than half an hour it became turbid, when over the fire. Letting it cool, I softened the end of the tube, and observed that the glass was pressed inwards. I then made it boil very violently about
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an hour, during which it grew more turbid. When it was cool, I observed that the turbidness was occasioned by very small white particles, which subsided, and left the liquor quite clear at the top. Softening the end of the tube again, it was driven outwards with great force, and blew out the candle; so that, upon the whole, there had been an increase of elastic matter within the tube, notwithstanding the precipitation.

After this, I placed in the sand furnace an alkaline liquor of my own preparing, by impregnating distilled water with alkaline air. It was confined in a long tube, a quarter of an inch in diameter, on the 3d of September, and on the 9th of the same month the tube was quite coated with a white substance, and the liquor was turbid. On the 30th of the same month it had deposited a white sediment, though it was still very turbid. There was also a similar incrustation at the surface of the liquor, and extending in streaks three inches above it. At the same time, that which had been bought at the apothecary's, and which had been placed in the same furnace exhibited the same appearance. In this the incrustation reached six inches above the surface of the liquor, especially on the side to which it had been inclined. One of these tubes remained in the hot sand till the 19th of January following, when

when I found it broken; five or six inches of the lower part of it being covered with a thick white incrustation.

Atmospherical air within one of the glass tubes, hermetically sealed, in which a quantity of water had been exposed several months, in a sand heat, was not at all injured by it; and the trial was made more than a year after an end had been put to the experiments with the sand heat.

A small quantity of the blue solution of copper in sal-ammoniac, being exposed to the heat of a common fire, in a long glass tube hermetically sealed, presently became green, and afterwards yellow.

In the preceding experiments I observed a remarkable deposit from the solution of *mercury*, and also that of *copper* in spirit of nitre. But they both require a considerable *time*. I afterwards found that the deposit from *iron*, and from copper in volatile alkali, are made much sooner.

In a glass vessel hermetically sealed, a good deal of iron was precipitated in the form of *red earth* from a weak solution of it in spirit of salt, placed in a sand heat, only a single day. It was also precipitated in great abundance from a solution of spirit of nitre in the same time. In this case the vessel was not quite closed, so that a little could

evaporate. The remaining liquor was quite colourless.

Copper was precipitated in the form of a deep blue earth, from a solution of it in volatile alkali at the same time; but the remaining liquor was almost as blue as at first. The solid precipitate smelled strong of the volatile alkali, as well as the solution itself.

I have likewise found a similar result with respect to a solution of copper in volatile alkali. In one day a similar deposit was made from this solution in the same circumstances. The substance deposited was of a dark blue colour, and adhered firmly to the glass; and when the vessel was first opened, there was a pretty strong smell of volatile alkali.

SEC.

SECTION IV.

Of the Colour given to Minium by Heat.

AS I was heating a quantity of minium in an iron ladle, I was very much struck with the resemblance of its colour, and of the change of its colour, to that of blood. The colour of good minium is, as nearly as possible, that of florid, or what I call, dephlogisticated blood. It is the colour they both acquire from exposure to the air. When the minium was in the ladle over the fire, the surface continued of this colour, but all the lower part of the mass was of a deep red, or black, the colour of dark coloured, or phlogisticated blood. But, like blood (only, in this case, the process was much quicker) the moment that any part of it was turned up to the open air, it resumed its florid light colour; and when it was cold, it could not have been perceived that any thing had been done to it.

Imagining that this dark colour might be the consequence of the minium receiving phlogiston from the iron, I exposed a quantity of it to the

same degree of heat in a glass tube, but found the same change of colour. In this, therefore, it resembles the change of colour in spirit of nitre, which is produced by heat only, without the help of any additional phlogiston, unless any may be supposed to pass through the glass.

The tube was several feet long, and was quite filled with the minium; and presently after it was exposed to the heat of the fire, the colour began to change, growing darker and darker continually, till it was almost black, exactly as it had done in the iron ladle. But when it was cold, it re-assumed its florid light colour. That it should do this without the access of the external air rather surprized me; and yet that no air, except what was contained in the interstices of the minium itself, had access to it, was evident from the lower part of the glass being ready to burst with the expansion of the air, when it was in a melting heat.

It was observable, that from the black colour, the minium passed, without any sensible interval, into yellow, in which state it contains little or no air of any kind; so that the florid colour is an indication of its containing pure air, whatever be the connexion between these circumstances. It must be observed, however, that minium deprived of its red colour by spirit of salt does not lose its property of yielding dephlogisticated air.

BOOK XII.

OBSERVATIONS RELATING TO THEORY.

SECTION I.

Of the constituent Principles of the different Kinds of Air.

IT is always our endeavour, after making experiments, to *generalize* the conclusions we draw from them, and by this means to form a *theory*, or *system of principles*, to which all the *facts* may be reduced, and by means of which we may be able to foretel the result of future experiments. With a view to this it has of late been a great object with philosophers to ascertain the number of *elements* that are necessary to constitute all the substances with which we are acquainted, and especially the different kinds of air, to which our attention has been much directed, in consequence of their seeming to bring us a little nearer to the ultimate con-

stituent parts of bodies ; finding that by their union they are capable of forming solid masses.

In my former publications I have frequently promised, and sometimes attempted, to give such a general theory of the experiments in which the different kinds of air are concerned as the present state of our knowledge of them enabled me to do, and I cannot well decline attempting something of the same kind in this new edition of all that I have published before ; though I acknowledge that I am very far from being able to satisfy myself with respect to it, and therefore cannot expect to give much satisfaction to others. When I published the first of my six volumes, I was not aware of much difficulty on this subject, but new experiments soon unhinged whatever I had thought the best established ; and this has been so often the case, that my diffidence increases in full proportion to the increase of our knowledge.

Fluctuating, however, as the present state of this branch of knowledge is, I shall not decline to give my present views of it ; nor shall I find any more difficulty in retracting any opinion I shall now advance, than I have hitherto done in retracting what I have advanced before. The sketch that I shall now give may at least serve, like former theories, to amuse us when we look back upon it, after having gained a more perfect knowledge of the subject.

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According to my latest observations, *water*, or rather *vapour*, is the basis of all kinds of air, or that to which they owe their peculiar kind of *elasticity*; so that all kinds of air may be said to be vapour with something else so attached to it, as to prevent its condensation in the temperature of the atmosphere.

The most simple of all the kinds of air are the *inflammable* and *dephlogisticated*; the former consisting of water and phlogiston, and the latter of water and something that may be called the *principle of acidity*, as it appears to be necessary to the constitution of all acids. Water seems to constitute about nine parts in ten of dephlogisticated air, but there seems to be a much less proportion of it in inflammable air.

The *hepatic air* of Mr. Bergman appears from late experiments to be sulphur dissolved in inflammable air, and *phosphoric air* to be phosphorus dissolved in it; because if either of these substances be melted in inflammable air, that species of air to which it gives a name will be formed. According to this theory, what I have called *sulphureous inflammable air* will be nothing materially different from hepatic air, that is, inflammable air at least partially saturated with sulphur, though I was not aware of it at the time of the discovery.

Oil of various kinds seems to be dissolved in inflammable air, so as to make the air burn with a lambent flame, of various colours. The variety of *smells* of which inflammable air is capable, shews that it admits of a great variety of impregnations; and this is not extraordinary, considering how nearly it approaches to a simple substance, as it contains only two elements, viz. water and phlogiston.

Fixed air seems to consist of about one half water, and the other half phlogiston, and dephlogisticated air in the proportion of one fourth of the former, to three fourths of the latter. It is formed by means of inflammable and dephlogisticated air, when either of them is disengaged by heat from the substance containing it, in the other species of air actually formed, or (which can hardly be said to make a different case) when the substances containing each of them are heated together; whereas if both the kinds of air were previously formed, and then decomposed together, they will make *nitrous acid*, with nothing more (and this only in some cases) than a very slight appearance of fixed air.

Nitrous air consists of phlogiston, and some portion of the *acidifying principle*, combined in a very peculiar and unknown manner, so that much difficulty still attends the theory of this kind of air. That it contains the acidifying principle, or some modification of it, is evident from its admitting a candle

candle to burn in it after long exposure to iron. And in this state, to which I have given the name of *dephlogisticated nitrous air*, it seems to want nothing but exposure to heat to convert it into proper dephlogisticated air.

That nitrous air contains the principle of acidity, is also probable from pyrophorus firing equally well in this kind of air and in dephlogisticated air. It cannot be the *water* only in them both that is the cause of this accension, because pyrophorus that has been ignited, gives out by exposure to heat one of the elements, at least, of dephlogisticated air, viz. that which is contained in fixed air. That nitrous air contains this principle, is farther evident from the very fine experiment of Mr. Milner, who produced nitrous air by passing alkaline air over substances containing dephlogisticated air in a red heat. See Phil. Trans. Vol. LXXIX. p. 300.

We know but little of the nature of *phlogisticated air*; but that it contains phlogiston, seems to be evident from its assisting to form nitrous acid with dephlogisticated air, in the remarkable experiment of Mr. Cavendish with the electric spark. The same may be inferred from nitrous air leaving a residuum (generally about one fourth of its bulk) of phlogisticated air in a variety of processes, especially when part of its water has been extracted from it

it by heating iron in it. Also, as the iron loses its phlogiston, and extracts nothing from nitrous air, besides water, it seems probable that the acidifying principle in nitrous air is left behind in this process, and therefore that this must be another constituent principle in phlogisticated air. It also follows from the same experiment, that phlogisticated air must contain all the phlogiston in the nitrous air, which was four times its own bulk, and also that of the iron.

The different species of *acid air*, seem to be those acids in the form of vapour highly phlogisticated, and combined with a certain portion of water. Besides this, the fluor acid air contains a portion of the *earth*, called *fluor crust*.

Alkaline air appears to consist of phlogisticated air and inflammable air, both by its decomposition with heat, and its formation from nitrous air and iron, either in my own slow process, with cold iron, or Mr. Milner's very curious one with iron red hot. But the secret of the combination of phlogisticated air and inflammable air, so as to constitute alkaline air, is altogether unknown; and we cannot be said to know much of the *nature* of a substance, when we know nothing more than the *elements* of which it is composed, and are wholly ignorant of the manner of their *combination*; since substances most remarkably

markably different from each other, appear to consist of the same elements in different proportions, and united in a different manner.

The *nitrous acid* appears from my late experiments to be the most simple of all the acids; since it is formed by the decomposition of dephlogisticated air and the purest inflammable air; and as the acidifying principle is the same in all the acids, especially the three mineral ones, it is probable that some peculiar *additional substance* may be necessary to constitute the vitriolic and marine acids, as well as the vegetable ones. But even these may differ from the nitrous in nothing more than a different combination of the same elements, so very little do we know of the internal constitution of substances.

The action of the *electric spark* upon different kinds of air, is not easily explained. As a permanent inflammable air is formed by it from any kind of oil, or caustic volatile alkali, it must be capable of giving this aerial form to the water and phlogiston contained in these liquors; but as a red heat will do the same thing, this effect may be produced by means of the mere *heat* communicated by the spark. And something communicated by heat, seems to enter as a constituent principle into every species of air, because the water in the worm tub is not heated when air is produced from the vapour of acids.

acids. The element of heat, therefore, called by Dr. Black *latent heat*, extremely obscure as the subject is, seems to enter into the composition of all kinds of air.

SECTION II.

Of the Doctrine of Phlogiston.

ACCORDING to Stahl, phlogiston is a real substance, capable of being transferred from one body to another, its presence or absence making a remarkable difference in the properties of bodies, whether it add to their weight, or not. Thus he concluded that oil of vitriol deprived of water, and united to phlogiston, becomes sulphur, and that the calces of metals, by the addition of the same substance, become metals. The air that has since been discovered in the calces of metals, makes no great difference in the system. For as oil of vitriol must part with its water, as well as imbibe phlogiston, in order to its becoming sulphur, so the calx

calx must part with its air, as well as imbibe phlogiston, in order to become a metal.

What is now contended for is, that in the oil of vitriol changing into sulphur, something is *lost*, and nothing *gained*; and also that a calx becomes a metal by the loss of air only. And did facts correspond on this theory, it would certainly be preferable to that of Stahl, as being more *simple*; there being one principle less to take into our account in explaining the changes of bodies. But I do not know of any case in which phlogiston has been supposed to enter into a body, but where there is room to suppose that *something* does enter into it.

What has been insisted upon, as most favourable to the exclusion of phlogiston, is the revival of mercury, without the addition of any other substance, from the *precipitate per se*. In this case it is evident that mere *heat*, either in a close retort, or in vacuo, is sufficient to revive the metal. And as what is expelled from this calx is the purest dephlogisticated air, it has been said that mercury is changed into this calx by imbibing pure air, and therefore becomes a metal again, merely in consequence of parting with that air.

But Mr. Kirwan explains this case in the following manner, which to me appears satisfactory. The metal, when exposed to a certain degree of heat,
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in contact with pure air, imbibes indeed the pure air, and nothing else, retaining the whole of its own phlogiston; so that then it may be said to contain fixed air, which is composed of phlogiston and dephlogisticated air; and that in a greater degree of heat, the latter is expelled while the former is retained; so that this calx was always possessed of phlogiston sufficient for its own revival.

But that mercury may be deprived of its phlogiston, so as to be incapable of becoming running mercury again by mere heat, is evident from my experiments on *turbith mineral*. For if this substance be exposed to heat in a very clean earthen vessel, the vitriolic acid so effectually carries away its phlogiston, that a great proportion of it is left a mere calx, capable of bearing any degree of heat without revival; and it can never become running mercury again, but by being heated in contact with inflammable air, or some other substance containing phlogiston. It is evident, therefore, that this calx, which is a dark red substance, sometimes hard, and sometimes powdery, is mercury deprived of its phlogiston, and something must enter into it before it can become a metal. Consequently, the metals are not simple substances, but phlogiston always enters into their composition. This, indeed, is evident from those of my experiments in which I produce

duce any of the metals from the calces, by heating them in inflammable air, which is imbibed by them.

Monf. Lavoisier, and many who follow him, are of opinion that what has been called phlogiston, is nothing more than one of the constituent parts of water, the other being the *principle of acidity*; and this doctrine of the composition and decomposition of water, has been made the basis of an entirely new system of chemistry, and a new set of terms has been invented, and appropriated to it.

It must be acknowledged, that substances possessed of very different properties, may, as I have said, be composed of the same elements in different proportions, and different modes of combination. It cannot therefore be said to be absolutely *impossible* but that water may be composed of these two elements, or of any other; but then the supposition should not be admitted without *proof*; and if a former theory will sufficiently account for all the *facts*, there is no occasion to have recourse to a new one, attended with no peculiar advantage.

Also, that phlogiston is an element in the composition of water, is, as I have more than once observed, not improbable, since water conducts electricity like metals and charcoal, into which the same principle enters, and because, when fresh distilled, it attracts dephlogisticated air from the atmosphere,

mosphere, which is the property of other bodies containing phlogiston. By this means it may, in fact, contain both the principles, of which, according to the new theory, it wholly consists; and in what degree it contains them, we cannot tell. For though heat may expel a part of them in the form of air, the force of this action may be limited, so that water boiled ever so long may retain much air, which only a *red heat* will discover, especially so intense a heat as electricity is known to communicate. But this is no argument against the doctrine of phlogiston, since it only proves that this principle is contained in water, more or less intimately combined, as well as in many other substances. This may serve as a general reply to the conclusions that Messrs. Van Troostwyck and Dieman have drawn for their very striking experiments on water, till they can be repeated and examined with the attention that they certainly deserve.

Finding the process for procuring air from water, by means of the *electric spark*, a very slow one, and liable to many accidents, I had recourse to a well glazed *hot earthen tube*, and then to a *burning mirror*, throwing the focus upon a piece of crucible covered with water. In all these three methods I procured air; but thinking to preserve them till I got a quantity sufficient for a few explosions, in order to see whether any *acid* would be the result, I
found

found they were all either completely, or very nearly, absorbed by the water, even water sufficiently saturated with air, so that it would not imbibe either dephlogisticated or inflammable air. Consequently, something was wanting to constitute this produce proper *permanent air*. In this view the experiment is extremely curious, and well deserves to be prosecuted. If I have a good sun in the course of the approaching summer, I shall not fail to attend to it.

It is said by Mr. Lavoisier, and his friends, that water must consist of inflammable and dephlogisticated air, since it may both be composed from them, and resolved into them again. But their experiments I have shewn not to authorize the conclusion that has been drawn from them. When dephlogisticated and inflammable air are decomposed by heat, both in my experiments and theirs, nitrous acid is always formed; and though this acid has been said to come from the phlogisticated air, which could not be wholly excluded in the process, it is manifest from several considerations that it could not have this source; especially as the same process will not at all decompose, or in the smallest degree affect, phlogisticated air. Besides, if phlogisticated air should contribute to the formation of this nitrous acid, it is most natural to suppose that it is effected by imparting phlogiston, of which it

principally consists ; and in this manner I doubt not it does contribute to the formation of nitrous acid in the experiment of Mr. Cavendish, the dephlogisticated air furnishing the principle of acidity, and the phlogisticated air phlogiston, as the inflammable air does in my experiment.

In what manner soever dephlogisticated and inflammable air be made to unite, they compose *some acid*, and in no case *pure water*. If iron (containing phlogiston) be heated in dephlogisticated air, or if precipitate per se (containing dephlogisticated air) be heated in inflammable air, fixed air is always formed ; whereas according to the modern hypothesis, water only ought to be produced in both the cases. That the fixed air should come either from the plumbago in the iron, or from the precipitate per se, is impossible on the account of the quantity of it. The precipitate that I made use of, contained no fixed air at all, and whatever plumbago there may be in iron, it is always retained in the calx, and does not enter into the inflammable air procured from it, because that inflammable air may be decomposed without producing any fixed air.

Water, they say, is completely decomposed when it is made to pass over red hot iron, the iron imbibing the acidifying principle, and the remainder going off in the form of inflammable air. But it is
unfor-

unfortunate for this hypothesis, that no substances will answer for this experiment, except such as have always been supposed to contain phlogiston, and that all these do answer. It is therefore much more probable, that the inflammable air is formed by the *phlogiston* from these substances, and the *water* with which it is then supplied as a base, and that if any part of the substance remain, and acquire weight, it receives that additional weight from water only.

Charcoal almost wholly vanishes in this process, which it probably does by its entering wholly into the air that is produced, and the fixed air that is found mixed with the inflammable air, only shews that charcoal contains all the elements of fixed air, the acidifying principle as well as phlogiston, and it has not been shewn that it does not.

Iron acquires weight in this process, but it appears to be from *water only*, because when, after this, it is heated in inflammable air, that air is imbibed, and nothing but the purest water is found in the vessel; whereas if this *iron slag*, or *finery cinder*, had contained the acidifying principle extracted from the water, the heating of it in inflammable air would be attended with the same phenomena as the heating of *precipitate per se* in the same kind of air, viz. the production of fixed air. But this is not the case; there being no mixture of fixed air in what remains of the inflammable air in which finery cinder

is heated, but always in that in which the precipitate is heated.

The same is perhaps still more evident from heating the two substances, *minium* and *massicot*, in inflammable air. If minium, which contains pure air, or the acidifying principle, be heated in inflammable air, the lead will be revived, and fixed air will be found in the vessel; but if massicot, or that minium from which its air has been expelled by heat, be used, though the lead will be revived, no fixed air will be found. The result will be the very same as when iron is revived from finery cinder in inflammable air.

Had the iron imbibed dephlogisticated air from the water, and not water itself, there seems to be no reason why fixed air should not be found in this, as well as in the exactly similar process with minium and precipitate *per se*. Also, it can never be supposed, that the addition which iron gains, of one third of its weight, is from air contained in steam, if it could be proved to contain any; because, if there be a sufficient quantity of iron, the whole of the water will be imbibed; so that, on this hypothesis, water must be nothing but dephlogisticated air condensed.

There is, I acknowledge, a great difficulty in explaining the experiment of iron first imbibing water, and parting with phlogiston, and again parting with
its

its water, and imbibing phlogiston, in circumstances of heat so nearly similar as those which I have described. It seems as if the affinity of iron to water and to phlogiston was each, in their turns, stronger than the other. To this I can only say, that the whole doctrine of affinities, as far as it is true, is founded on facts; and these are clearly such as I have represented; and that a difference of circumstances, which is not apparent at present, may become so when we shall have given sufficient attention to them.

The results of the experiments with minium, compared with those in which finery cinder was used, not having been recited before, I shall introduce them here. In both these cases the residuums of the inflammable air were equally free from fixed air; and when they were fired with equal quantities of dephlogisticated air, the diminutions of bulk were very nearly the same, less than when the original inflammable air was used, because all the impurities in the whole quantity were retained in a small residuum, the metals having imbibed nothing but pure phlogiston. Also the inflammable air had been long confined by water, in consequence of which it is always altered more or less. The particulars of the processes were as follows:

The finery cinder was revived in 7 oz. m. of inflammable air, which was thereby reduced to $1\frac{1}{2}$ oz. m.; and an oz. m. of this residuum being fired together with an equal quantity of dephlogisticated air, not very pure, the diminution of both was to 28 divisions of a tube, of which 30 was one oz. m. when with equal quantities of the same dephlogisticated and the original inflammable air, the diminution was to 18.

The massicot was reduced in 8 oz. m. of inflammable air till it was reduced to $1\frac{1}{2}$ oz. m.; and after the process with the dephlogisticated air, the diminution was to 29, when with the original inflammable air it was to $17\frac{1}{2}$.

In both the residuums after the explosion, there was a slight appearance of *fixed air*, though none could be perceived before the explosion; but in both cases it was so slight that it could not have been perceived by the diminution of its bulk. But since both fixed air and nitrous acid are produced from the same materials in different circumstances, it cannot be thought extraordinary if, in some cases, both should be produced at the same time.

Another argument against the antiphlogistic doctrine, may be drawn from some experiments which I made upon Prussian blue, if the small quantity of fixed air that can be expelled from it by heat, be compared

pared with the much greater quantity when it is heated in dephlogisticated air.

According to Mr. Lavoisier, finery cinder contains nothing besides iron, and the principle of acidity. But if this be the case, and if charcoal, as they maintain, be a substance that contains no phlogiston, but is only capable of forming inflammable air by its assisting to decompose water, the heating of this *dry cinder*, together with *dry charcoal*, ought not to produce inflammable air, which however it does in great abundance; whereas this fact agrees exactly with the common hypothesis, the cinder, in return for the phlogiston it receives from the charcoal, giving out the water which it had before imbibed; and this water enabling the remainder of the charcoal to take the form of inflammable air.

It has been said, that if the finery cinder contained nothing but water, it could not be formed by heating iron in dephlogisticated air, which however is done with as much certainty as in pure steam. But by far the greatest part of the weight of dephlogisticated air is water, and the air being decomposed in the process, the water is imbibed by the iron, and the acidifying principle contributes to form fixed air, with the phlogiston, which is at the same time expelled from the iron; a fact which cannot be accounted for on the new hypothesis, which admits of nothing in the iron, which, by its combination

with dephlogisticated air, or any constituent part of it, *can* form this fixed air.

If water be not decomposed, both metals and sulphur do certainly yield inflammable air, when steam is made to pass over them in a red heat. They cannot, therefore, be *simple substances*, as the antiphlogistic theory makes them to be. Also, the same thing that they have parted with, *viz.* inflammable air (or rather something that is left of inflammable air when the water is taken from it, and which may as well be called *phlogiston* as any thing else) may be transferred to other substances, and thus contribute to form any of the metals, sulphur, phosphorus, or any thing else that has been deemed to contain phlogiston. This phlogiston, also, no doubt, having weight, it perfectly corresponds to the definition of a *substance*, having certain affinities, by means of which it is transferred from one body to another, as much as the different acids.

If there be no such thing as one principle of phlogiston, transferable from one substance to another, and the doctrine of the decomposition of water be denied, it must be admitted, that inflammable air from sulphur is real sulphur and water, that from iron, iron and water, as well as that very different substance, the *scale of iron*. And since copper, or any other metal, may be made of inflammable air from iron, &c. all the metals will be, in fact, convertible

vertible into one another. At least, it may be said, that all the component parts of any one metal may be so incorporated with any other, that no test can detect it. Also iron, made of inflammable air from sulphur, ought, upon this hypothesis, to have the properties of *sulphurated iron*, which undoubtedly it would not have. An hypothesis loaded with these difficulties must be inadmissible; whereas that of phlogiston is extremely simple, and, as far as appears, of universal application.

The discovery that the greatest part of the weight of inflammable air, as well as of other kinds of air, is water, does not make the use of the term phlogiston less proper: for it may be still given to that *principle*, or thing, which, when added to water, makes it to be inflammable air; as the term *acidifying principle* may be given to that thing which, when it is incorporated with water, makes dephlogisticated air.

SEC-

SECTION III.

A more particular Answer to the Objections of the Antiphlogistians.

IT will be expected, that in this reply to the objections that have been made to my experiments establishing the doctrine of phlogiston, I should consider what has been alledged by Messrs. Lavoisier, Berthollet, and de Fourcroy, in favour of their new system, in their *Report* on the subject of the new chemical characters invented by Messrs. Hassenfratz and Adet, subjoined to the new *Nomenclature Chimique*. I shall therefore notice what appears to me to be most important in that publication.

“ One of the articles of the modern doctrine” of which they say, p. 311, “ that it cost more than “ twenty years labour, which the force of reasoning has obliged many celebrated chemists to “ adopt, and in favour of which much greater numbers are ready to decide ;” (and the evidence for which they say, p. 301, “ is the most complete “ chemical proof) that seems the most solidly “ established,” p. 298, “ is the formation, the de-

“ composition, and recomposition of water; and
“ how is it possible,” they add, “ to doubt of it,
“ when we see that, in burning together fifteen
“ grains of inflammable air and eighty five of pure
“ air, we get exactly a hundred grains of water;
“ and when we can, by decomposition, find again
“ these same two principles, in the same propor-
“ tions?”

To this I must say, as I did, when I was myself a believer in the decomposition of water, that I have never been able to find the full weight of the air decomposed in the water produced by the decomposition; and that now I apprehend it will not be denied, that the produce of this decomposition is not mere water, but always some acid.

M. Lavoisier and his associates farther observe, p. 300, with respect to my experiments, that “ when
“ a calx is revived in inflammable air, more water
“ is found in the vessel than the weight of inflam-
“ mable air that disappears, so that it could not
“ have been contained in that air.” They only refer to my experiments in general; but as they speak of the water produced as appearing both on the inside of the vessel, and on the surface of the mercury, it can be no other than the experiment of the revival of iron from finery cinder; and the water that is found in this process was never supposed to come from the little that is contained in
the

the inflammable air, but the much greater quantity contained in the cinder.

I shall also consider the farther objections that have been made to the doctrine of phlogiston, and to my experiments in favour of it, that have since been made by Mr. Berthollet, in an elaborate *Memoir* contained in the *Annales de Chymie*, vol. III. p. 63, &c.

To the experiment with the finery cinder and charcoal Mr. Berthollet objects, p. 79, that I probably got more fixed air than inflammable, that the inflammable air contains much charcoal dissolved in it, and that in many experiments charcoal appears to retain water very obstinately.

How obstinately charcoal retains water, is easily ascertained. For Mr. Berthollet himself would say, that when any particular degree of heat would not make charcoal yield any more inflammable air, there was no more water retained in it than the same degree of heat was able with its assistance to decompose. But by the assistance of finery cinder, with even a much less degree of heat, it yields inflammable air very copiously, just as if steam had been made to pass over it in that heat; and judging from evident appearances, there can be no doubt but that, with a sufficient quantity of finery cinder, to supply it with water, all the phlogiston in the charcoal, exclusive of that which contributed to the revival

revival of the iron, will be converted into inflammable air. As to the proportions between the fixed air and inflammable, and that of the charcoal, which he supposes to be combined with the inflammable air, they are nearly the same in that inflammable air which is formed from charcoal by water.

To my experiment with the *terra ponderosa*, which proves that water is a constituent part of fixed air, and therefore probably of other kinds of air also, Mr. Berthollet objects, p. 82, that I did not examine the loss of weight in this substance. But after the process it adhered so closely to the earthen tube in which the experiment was made, that the loss of weight cannot be ascertained with accuracy. But this is not at all necessary. I found very exactly how much fixed air a given quantity of this substance would yield by means of water, which appeared to be the very same that it yielded by solution in spirit of salt, and that it yielded no air at all by mere heat without water. It was quite sufficient therefore to find how much water was expended in procuring any quantity of fixed air from this substance. And as there was no other source of loss of water besides the fixed air, it could not but be concluded, that it entered into its composition, as a necessary part of it, and in the proportion which I ascertained.

Mr. Berthollet in this memoir takes it for granted that, in the decomposition of dephlogisticated
and

and inflammable air in the copper tube, the acid came from the phlogisticated air, which I acknowledge that I could not wholly exclude, merely because dephlogisticated and phlogisticated air produced the same acid in Mr. Cavendish's slow and very different process by the electric spark, without considering my repeated answer to this objection, viz. that it always appeared by actual trial, that any given quantity of phlogisticated air, purposely mixed with the two other kinds of air, always remained intirely unaffected by this process; that the more phlogisticated air that was mixed with the dephlogisticated air, the less acid I constantly got, and that the purer the dephlogisticated and inflammable airs were, the more acid I got.

He also supposes that in this process I first procured a *sulphureous acid*, and that it became, by imbibing pure air from the atmosphere, a proper *nitrous acid*. But as the experiment was made in a close copper tube, and in general with no superfluity of dephlogisticated air, there was no opportunity of the liquor attracting any. Besides it is not at all material *which* of the nitrous acids be formed. That a great proportion of it is of a highly phlogisticated kind is acknowledged, and on this account it is that it so easily makes its escape, so as to have deceived the advocates for the antiphlogistic theory.

has
Mr.

Mr. Berthollet says, p. 87, that I explain the difference between my process and that of Mr. Cavendish by the *difference of temperature*; whereas no such idea ever occurred to me. It is probable, indeed, that the heat communicated by the electric spark is much greater than by the simple ignition of dephlogisticated and inflammable air. But if this be the reason why phlogisticated air is decomposed in Mr. Cavendish's process and not in mine, which I am far from denying (because in most other cases of the effect of electricity on air, it seems to act by the mere communication of *heat*, since in most other cases heat communicated in a different manner will produce the same effect) still it is a *degree* of heat that is communicated in the one case, and by no means in the other. As yet we know of no heat equal to that of the electric spark, and this may be the reason why it is able to decompose common air, which no other heat is.

He supposes, p. 89, 90, that the experiment in which I procured fixed air from the precipitate per se with which he obligingly furnished me, was by explosions in the copper tube; whereas it was that with the burning lens, a process totally different. By the one I uniformly produced nitrous acid, and by the other fixed air.

The precipitate per se with which Mr. Berthollet furnished me, he says, p. 91, contained a considerable

siderable quantity of fixed air; and yet he allows, that when admitted to lime water it did not immediately make it turbid, which it is well known a tenth part of the fixed air which I procured by means of it would have made it instantly and completely white. The turbulency that came on afterwards must therefore have had some other cause, probably some acid of vitriol in the water of the trough in which the experiment was made, and which gradually insinuating itself into the lime water in his tube, would make *selenite*, a thing that has frequently occurred in the course of my own experiments, and which for some time puzzled me not a little.

The quantity of fixed air produced by heating substances in inflammable or dephlogisticated air with the burning lens, Mr. Berthollet supposes, p. 93, I over-rated, by measuring it in a heated vessel. But the quantity of air was always measured in a separate vessel, and in the very same temperature in which the air on which I operated was measured. There was therefore no danger of my making it more than it really was, and though he says it was no more than the precipitate per se would have yielded, I do not hesitate to say that it was *infinitely* more, because the precipitate of itself yielded *none at all*.

I must

I must observe, that what I have usually called *finery cinder* (because it was so called in the furnaces, and I am not fond of giving new names to things) Mr. Berthollet always calls an *oxide of iron*, and he also calls *massicot* an *oxide of lead*, taking it for granted, that they consist of those metals united to the principle of acidity, and he thinks that my producing only water from heating them in inflammable air to be a presumption that no other oxide can do any thing more. But I have abundantly shewn, that the finery cinder contains no acidifying principle at all, and it is expelled from minium when it becomes massicot. Those metals therefore are revived by inflammable air from this kind of calx without producing any fixed air, to the composition of which dephlogisticated air, or the acidifying principle in it, is necessary. He says, p. 96, that the heat by which minium becomes massicot cannot change its nature. But this it evidently does by expelling from it all its dephlogisticated air, and by this means deprives it of its power of generating fixed air when it is heated in inflammable air. How else can Mr. Berthollet explain the production of fixed air in this process by means of *minium*, and not by means of *massicot*?

Inflammable air from iron, Mr. Berthollet says, p. 97, contains more or less of charcoal from the plumbago contained in it; and that this is the

source of the fixed air that I have found in the decomposition of it. But it may not only be washed in lime water, but even be wholly decomposed by being fired together with dephlogisticated air, without discovering any fixed air at all, so that it is impossible that it should have contained any. Also it is highly improbable *a priori* that inflammable air from iron contains any thing from plumbago. In all the solutions of iron in acids the plumbago is left undissolved, and that indeed is the only method by which we can estimate the quantity of it; and when inflammable air is procured from iron by means of steam, the case is no doubt the same; because the inflammable air procured in this manner appears in all experiments to have the same properties with that which is procured by means of acids. I always used the purest malleable iron till I found that there was no difference whatever between the air procured from this, and that from cast iron by means of steam. Had plumbago at all entered into the inflammable air, there must have been a greater quantity of it in that from cast iron than from the malleable, since the former contains so much more plumbago.

M. Berthollet objects to my observation, that the weight of the liquor which I produced from the decomposition of dephlogisticated and inflammable air was never equal to the weight of the air, which

difference I account for by the escape of the phlogisticated acid, because I took no account of the *residuum* of the air in the vessel in which I made the explosions. But if he reads my paper with more attention, he will find that I did *not* overlook this circumstance, since I measured the capacity of the vessel by the quantity of air that actually disappeared; by having been completely decomposed in the process; so that there was no occasion whatever to take an account of the air that was not affected by it.

These are all the objections that I have yet heard to the doctrine of *phlogiston*. The reader will judge of the force of them, and also of my replies. As I have been more than once upon the point of abandoning it; and in my sixth volume actually declared in favour of the *decomposition of water*, I should not feel much reluctance to adopt the *new doctrine*, provided any new and stronger evidence be produced for it. But though I have given all the attention that I can to the experiments of M. Lavoisier, &c. I think that they admit of the easiest explanation on the *old system*.

ARTICLES OMITTED.

In the New Arrangement of the Materials of Six Volumes, it will not be thought extraordinary that a few Omissions should be made. The following are all that I have observed.

Vol. I. p. 128, after the last paragraph, add—

LASLTY, I would observe that Sir Wm. Lee was so obliging as to communicate to me a very useful discovery of his, viz. to keep flesh meat a long time sweet, even in hot weather, by frequently washing it with water impregnated with fixed air, as is particularly related by himself, in the Appendix to Vol. II. p. 461.

Vol. II. p. 347, after the second paragraph, add—

Water probably, and *alkaline air* certainly, require the same quantity both of vitriolic acid air, and of fluor acid air, to saturate them. *Water*, I have observed, imbibes about ten times more marine acid air than it can of vitriolic acid air.

In order to try the power of water to imbibe fluor acid air, I put six grains of water into a small
glass

glass tube, closed at one end, into a jar of fluor acid air, and seven grains of water to another jar of the same air. The former imbibed one ounce measure of the air, and the latter two ounce measures. This was in a wider vessel, which was probably an advantage with respect to the absorption of the air. This absorption, however, though certainly greater than in the case of vitriolic acid air, is far short of the quantity that would have been imbibed of marine acid air; and I afterwards found that the *fluor crust* itself imbibes a considerable quantity of this acid air; so that it is possible, that, exclusive of this absorption by the fluor crust, the *water* might not have imbibed more of this, than it would have done of vitriolic acid air.

Ibid, p. 349. after the title of Section II,

The mixture of any other of the acid airs, with *alkaline air*, makes so beautiful an experiment, that it was naturally one of the first that I thought of making with this new acid air. Accordingly, I got the appearance that I had expected; a white cloud being formed by the union of these two kinds of air. But the alkaline air did not mix so readily with this as with the other kinds of acid air; and which surprized me much at the time, the salt formed by the union of these two kinds of air was not soluble, either in water or spirit of wine.

But, in fact, the proper *salt* formed by the union of these kinds of air was, no doubt, dissolved in the water; that which remained undissolved being, as I conjecture, the *stony substance* only which had been held in solution in the acid air. This stony substance being mixed with the acid air, is also probably the reason why the alkaline air does not mix so readily with it as with the other kinds of acid air; some time being requisite to disengage it from this stony substance, in order to its uniting with the alkaline air.

INDEX

AN ALPHABETICAL

I N D E X

TO ALL THE THREE VOLUMES.

N. B. The Roman figures denote the *volumes*, and the other the *pages*. When no volume is expressed, the first volume, or the volume last mentioned, is to be understood.

A

ACETOUS fermentation, fixed air produced in it, 126

Acid airs, all the kinds of them mixed with fixed air, 383

Acids, absorb nitrous air, 381; impregnated with nitrous vapour, iii. 144

Aerial form of substances, difficulties concerning it, ii. 402

Air, former discoveries relating to it, i; in the hollow parts of plants, ii. 202; in sea weed, iii. 275; produced rapidly or slowly, ii. 438; kinds of it that have no mutual action mixed, 441; expanded by heat, 448; their specific gravity, 451; found in them, 453; conductors of heat, 457; their refractive power, 460; in the bladders of fishes, 462; exposed to urine, 463; in the calces of metals, 469; supposed to be contained in mercury, 471; not absorbed in

making nitrous acid, iii. 1; absorbed by the willow plant, 331; extracted from charcoal, 416; the elements of which all the kinds are composed, 533

Alkali, caustic, absorbs nitrous air, 378

—, *volatile*, produced from nitrous air and iron, ii. 41; converted into inflammable air by the electric spark, 198

Alkaline air, discovered, ii. 368; water impregnated with it, 374; mixed with other kinds of air, 575; with liquids, 377; with solid substances, 378; lighter than acid air, 381; mixed with all the kinds of acid air, 383; the electric spark in it, 389; analyzed 396; lead, &c. revived in it, 356, 398; mixed with vitriolic acid air, ii. 314

Aloes, air from it, 83

Ammoniac, nitrous, formed by nitrous air, 398.

Animal substances, air from them by heat, 94; how affected by spirit of nitre, iii. 86; producing green vegetable matter, 322.

Apparatus, for experiments on air, 12

Aqua Regia, a new kind of it, iii. 151

Argand, Mr. his lamp recommended, 41

Arsenic, inflammable air from it by heat and steam, 204

Asbes, attract fixed air, 137

— of *pit-coal*, air from them, 91; of wood do. 90

Atmospherical air, by what processes not injured, ii. 189; by what it is injured, 203; by iron and sulphur, &c. 203; by fumes of charcoal, 206; by calcination of metals, 209; by putrefaction, 216; by calces of copper and iron, 219; by vapour of mercury, 225; by oils, 227; by oil of turpentine, 232; by spirit of nitre, 236; by water fresh distilled, 243; by flowers, 247; by the electric spark, 248; by putrid marshes, 253; by inflammable air, 266; its purity in different circumstances, 259; restored by vegetation, iii. 247, 273, 293

B

Balloons, the cheapest method of filling them, 291

Basalts, air from it, 64

Bath water, air from it, 59

Belemnites, air from it, 78

Bile, imbibes nitrous air, 396

Black, Dr. his discoveries concerning fixed air, 4

—, powder from mercury, iii. 432

Bladder, air acting through it, 174, iii. 388

Blood, air from it, 99; use of it, iii. 348

Bones, air from them by steam, 300

Boyle, Mr. his discoveries concerning air, 3

Browning, Dr. his observations on air in Pyrmont water, 4

C

Calces of metals, imbibe inflammable air, 248; revived in alkaline air, 256; air from them, ii. 469

Calcination of metals, injures air, ii. 209, 219

Candles, the burning of them injures air, ii. 213

Carols, inflammable air from them, 211

Cavendish, Mr. his discoveries concerning air, 5

Chalk, air from it, 72

Charcoal, air from it and finery cinder, 204, 297; absorbs inflammable air, 223; air from it by steam, 284; heated in nitrous air, ii. 38; fumes of it injure air, 206; heated in dephlogisticated air, iii. 377; its conducting power, iii. 396; its expansion by heat, 410; air from it, and imbibed by it, 414

— of metals, iii. 425; of copper heated in dephlogisticated air, 164, iii. 377

Cigna, Mr. his discovery concerning the redness of the blood, iii. 359

Clay, air from it, 71, 80

Clyffas, of nitre, air from it, 352

Coak, air from it by steam, 303

Coal pit, state of the air in it, 79

Comfrey, growing in inflammable air, iii. 337

Conducting power, of certain substances, iii. 512

Copper, the firing of paper dipped in

in a solution of it, iii. 203; calx of it heated in inflammable air, 489; solution of it in spirit of nitre exposed to a continued heat, 521, 529

Copperas, air from it, 85

D

Dephlogificated air, enters into the composition of fixed air, 145; the discovery of it, ii. 102; produced by spirit of nitre and the calx of lead, 120; from various kinds of earth, 128; from vitriolic acid and the metals, 141; do. and other substances, 149; from several mineral substances, 154; combustion and respiration in it, 160; great purity of it, 170; procured in large quantities from nitre, 173; white matter deposited from it, 178; plants growing in it, iii. 276; quantity of it consumed in respiration, 375; emitted from green vegetable matter, 282

Dephlogificated nitrous air, the discovery of it, ii. 54; from the solution of metals in nitrous acid, 58; by iron filings and sulphur, 70; by iron in a solution of copper, 76; separated from phlogificated air, 81; converted into dephlogificated air, 89; its constitution, 97

Detonation, explained, ii. 181

Dining rooms, state of the air in them, ii. 264

Duck weed, in inflammable air, iii. 337

E

Electric spark, in fixed air, 112; in nitrous air, ii. 22; in dephlogificated nitrous air, 92; in common air, 248; in marine acid air, 293; in vitriolic acid air, 323; in alkaline air, 389; in various liquids, iii. 508

Ether, inflammable air from it by electricity, 196; by heat, 201; saturated with marine acid air, ii. 286; impregnated with nitrous vapour, iii. 138; various experiments on it, ii. 436

F

Finery cinder, air from it and charcoal, 204, 297; various experiments relating to it, iii. 504

Fishes, their respiration, iii. 382; in water impregnated with fixed air, 386; air in their bladders, ii. 462

Fixed air, water impregnated with it, 43; preserving flesh meat, iii. 564; directions for the process, 48; fatal to animals, 100; to vegetables, 101; the electric spark in it, 112; its acidity proved, 119; expelled from water by boiling, 120; the freezing of water saturated with it, 120; how affected by iron filings and sulphur, 121; changed by being incorporated with water, 123; exposed to heat, 125; a source of deception from its being contained in water, 125; in acetous fermentation, 126; from putrid mice, 127; water essential to it, 129; procured from nitrous acid, 133; from the atmosphere, 136; from the vitriolic acid, 142; composed of dephlogificated air and phlogiston, 145; iii. 377; by heating substances containing phlogiston in dephlogificated air, i. 159; from substances containing dephlogificated air in inflammable air, 167; not contained in minium, &c. 172; procured by inflammable or nitrous air acting on dephlogificated air through a bladder, 174

Flesh meat, inflammable air from the

- the putrefaction of it, 216; preserved by water impregnated with fixed air, iii. 364
Flowers, their effluvia injure air, ii. 247
Fluor acid air, the discovery of it, ii. 339; water saturated with it, 342; the constitution of it, 349; various substances exposed to it, 353; miscellaneous experiments on it, 363; corrodes glass when hot, 366; mixed with alkaline air, iii. 365
Franklin, Dr. his letter on the restoration of air by vegetation, iii. 269
Freezing, of water impregnated with vitriolic acid air, 359
Fruits, inflammable air from them, 213

G

- Gas*, an unnecessary term, 9
Granite, air from it, 67
Green vegetable matter, giving pure air, iii. 282; its natural history, 306; from vegetable substances in water, 312; from animal substances, 322
Guns powder, air from it, 351; fired in different kinds of air, iii. 205; a production similar to it, 206
Gypsum, air from it, 69

H

- Hales*, Dr. his discoveries concerning air, 5
Heat, its effects on different kinds of air, ii. 448; how conducted by them, 457; long continued experiments with it, iii. 516
Holmes, Van, his discoveries concerning air, 2
Hot-bousses, state of the air in them, ii. 263

I

- Ice*, in marine acid air, ii. 293
Inflammable air, observations on

it, 182; from metals, 182; affects common air in its nascent state, 187; from oil, 195; do. of turpentine, 198, 228; from various substances by heat in water, 200; do. putrefying in water, 206; in mercury, 216; absorbed by charcoal, 223; putrefaction in it, 224; plants growing in it, 224; water impregnated with it, 225; animals dying in it, 229; changed by standing in water, 230; the electric spark in it, 232; different smell of it, 233; decomposed in hot flint glass tubes, 234; sulphurated, 241; imbibed by the calces of metals, 248; sulphur produced by means of it, 264; phosphorus do. 262; nitrous air do. 263; contains water, 266; from charcoal and iron by means of steam, 280; from bones, 300; from zinc, 301; from coak, 303; the phlogiston contained in it compared with that in nitrous air, 305; analysis of different kinds of it, 308; burned with nitrous air, 408; how it affects common air, ii. 266; from alkaline air, 389; fired in the vapour of nitrous acid, iii. 177; from iron in its different states, 491

—, *sulphurated*, 241
Iron, exposed to fixed air, 122; inflammable air from it by steam, 288; heated in nitrous air, ii. 38, 49; procuring dephlogisticated nitrous air by a solution of copper, 76; that has been used to diminish nitrous air, 93; various experiments relating to it, iii. 480; heated in dephlogisticated air, *is*. inflammable air from it in different states, 491; annealed, 493;

pre-

precipitated from its solutions by heat, 329

Iron ore, air from it, 74

Iron filings and sulphur, in fixed air, 121; air from it, 184; diminishes nitrous air, ii. 6; produces dephlogisticated nitrous air, 71; affects common air, 203

L

Landriani, Sig. his observations on volcanic fires, ii. 158

Lane, Mr. his discovery concerning water impregnated with fixed air, 5

Lava, air from it, 64

Lavoisier, Mr. his theory considered, iii. 554

Lead, white, air from it, 86

Lee, Sir Wm. his account of preserving flesh meat by water impregnated with fixed air, iii. 564

Light, its effect on phlogisticated nitrous vapour, iii. 126; its influence on the production of pure air from plants, 293

Lime stone, air from it, 71

Liver of sulphur, produces dephlogisticated nitrous air, ii. 73; injures common air, 205

M

Macbride, Dr. his observations on fixed air, 4

Manganese, air from it, ii. 154; by steam, i. 303

Marine acid, its colour, iii. 208; discharged, 221; various substances saturated with it, 215; effect of continued heat upon it, 227; dephlogisticated, 235; the electric spark taken in it, iii. 509

Marine acid air, the discovery of it, ii. 275; its effects on substances containing phlogiston, 280; on substances containing

the stronger acids, 289; on ice, 292; extinguishes flame, 293; the electric spark in it, *ibid.*

Marshes, putrid, injure common air, ii. 253

Massicot, heated in inflammable air, iii. 550

Mercury, some appearances in the solution of it explained by nitrous air, 403; the vapour of it injures common air, ii. 225; the air supposed to be contained in it, 471; in vapour, conducts electricity, iii. 513; the solution of it exposed to continued heat, 521; agitated in water, 446; in spirit of wine, 455; in acid liquors, 461; its three states, 456; how affected by long agitation, 464; its great volatility, 470

Mercury, the black powder from it and lead, iii. 432; air from it, i. 151

Metals, inflammable air from them, 182; the charcoal of them, iii. 425; the proportion of phlogiston in each of them, i. 258

Mice, observations concerning them, 17; air yielded by them, 97, 127; by putrefying in mercury, 219

Mineral substances, air from them by heat, 63

Minium, impregnated with nitrous vapour, iii. 167; its colour, how affected by heat, 531

Molybdena, air from it 75

Monge, Mr. his observations on the electric spark in fixed air, 116

N

Nitre, the quantity of dephlogisticated air from it, ii. 173; the formation of it, 187; injures air, 240

Nitrous acid, convertible into fixed air, 133; yields pure air, ii.

120, 128; the vapour of it injures air, 236; absorbs nitrous air, 382; produced by decomposing nitrous air and dephlogisticated air, 28; observations on the process for making it, iii. 1; its different colour and strength, 9, 15; the phlogistication of it, 31; composed from dephlogisticated and inflammable air, 42; its action on vegetable substances, 65; on animal substances, 86; inflammable air fired in the vapour of it, iii. 177; mixed with vitriolic acid, 184; miscellaneous experiments on it, 203; the electric spark taken in it, 409

Nitrous air, how mixed with common air, 26; formed from inflammable air, 263; the phlogiston in it and inflammable air, 305; the discovery of it, 328; the quantity of it from different metals, 330; from galls, 334; from vapour of spirit of nitre and water, 335; the quantity of it increased by the acid being previously converted into vapour, 341; from phlogisticated nitrous acid, 347; a test of the purity of air, 354; old and fresh made compared, 363; water impregnated with it, 364; absorbed by oils, 372; by alkalis, 378; by spirit of wine, 380; by acids, 381; its antiseptic power, 391; imbibed by bile, 396; forms nitrous ammoniac, 398; explains some appearances in the solution of mercury, 402; the freezing of water impregnated with it, 407; burned with inflammable air, 408; plants and animals in it, 409; recommended to be used in glysters, 410; diminished by standing in water, ii. 1; not affected by steam,

5; diminished by iron filings and sulphur, 6; by a solution of green vitriol, 8, 20; by charcoal, 18; by long standing in water, *ib.* by pyrophorus, 19; by the electric spark, 22; produces nitrous acid with dephlogisticated air, 28; contains water, 34; iron heated in it, 38, 49; produces volatile alkali, 41; its constituent principles, 46; contains no nitrous acid, 47; pyrophorus fired in it, 50; diminished in a bladder, iii. 388

Nitrous air, dephlogisticated, ii. 13; heating iron and charcoal in it, 38

Nitrous vapour, phlogisticated, makes nitrous air with water, 335, 342; how produced, iii. 100; observations on it, 115; imbibed by animal oils, 106; influence of light upon it, 126; water impregnated with it, 129; oil and spirit of wine impregnated with it, 136; acids do. 144; oil of vitriol do. 156; solid substances do. 165

Nooth, Dr. his method of impregnating water with fixed air, 55

O

Odours, do not assume the form of air, ii. 406

Oil, inflammable air from it by electricity, 195; absorbs nitrous air, 372; injures common air, ii. 227; impregnated with phlogisticated nitrous vapour, iii. 136
—, *animal*, imbibe phlogisticated nitrous vapour, iii. 106

Onions, inflammable air from them, 207

P

Parker, Mr. his merit in constructing burning lenses, 173

Parfnips, inflammable air from them, 211

Perspiration,

Perspiration, does not injure air, ii. 192

Phlogisticated air, observations concerning it, ii. 188; effect of water upon it, 271

Phlogiston, in fixed air, 145, iii. 377; the proportion of it in inflammable and nitrous air, i. 305; the doctrine of it defended, iii. 540

Phosphoric acid, observations on it, iii. 240; the electric spark in it, 510

Phosphorus, heated in dephlogisticated air, 170; produced by inflammable air, 262

Plants, in fixed air, 101; in inflammable air, 224; in dephlogisticated air, iii. 276

Precipitate per se, heated in inflammable air, 168; formed by dephlogisticated air, ii. 185

Pringle, Sir John, letter to him concerning putrid marshes, ii. 254

Prussian blue, heated in dephlogisticated air, 160

Putrefaction, injures air, ii. 216

Pyrophorus, in nitrous air, ii. 50; injures common air, 206; imbibes air, iii. 423

R

Radical vinegar, air from it with whiting, ii. 153

Respiration, experiments on it, iii. 348; of fishes, 382

Rowley rag, air from it, 66

Rusting, of metals in air, ii. 186

S

Saline substances, air from them, 81

Schistus, air from it, 70

Sea weed, air contained in its bladders, iii. 279

Seltzer water, observation on it at the well, 62

Spirit of wine, inflammable air from it by electricity, 197; by heat, 200; absorbs nitrous air, 380;

not convertible into air by boiling, ii. 405; impregnated with phlogisticated nitrous vapour, iii. 141; mercury agitated in it, 455

Steam, its action on various substances by heat, 301; air exposed to it, ii. 200; its conducting power, iii. 512

Steatites, air from it, 76

Steel, air from it, iii. 500

Stones, air from them, 77

Sugar, air from it, 90

Sulphur, inflammable air from it by steam, 203; in marine acid air, ii. 283; formed from vitriolic acid air, 333

T

Tartar, air from it, 87

Terms, observations on the use of them, 8

Terra ponderosa, used to prove the presence of water in fixed air, 130

Theory, observations relating to it, iii. 533

Tin, in spirit of nitre, ii. 65

Toadstone, air from it, 67

Turbith mineral, heated in dephlogisticated air, 169

Turnips, inflammable air from them, 212

Turpentine, spirit of it, air from it by electricity, 198; by heat, 201; inflammable air in it, 228; absorbs air, ii. 232

U

Urine, air from it, 98; its effects on inflammable air, &c. 462

V

Vapour, how distinguished from air, 11

Vat, fermenting, the air incumbent upon it, 44

Vegetable substances, how affected by spirit of nitre, iii. 65; produce

- duce green matter, 313; air from them, 87
- Vegetables*, in fixed air, 101; in dephlogisticated air, iii. 276
- Vegetation*, restores air injured by combustion, iii. 247; by putrefaction, 255
- Vinegar*, mercury agitated in it, iii. 462
- , *radical*, not convertible into air by boiling, ii. 402
- Vitriol, green*, a solution of it diminishes nitrous air, ii. 9
- Vitriolated tartar*, air from it, 82
- Vitriolic acid*, fixed air from it, 142; yields pure air, 141, 149; from vitriolic acid air, ii. 330; impregnated with nitrous vapour, iii. 156; mixed with nitrous acid, 184; the electric spark taken over it, iii. 509
- Vitriolic acid air*, the discovery of it, ii. 295; from metals, 301; water impregnated with it, 307; compared with marine acid air, 313; mixed with alkaline air, 314; with other kinds of air, 316; substances containing phlogiston exposed to it, 318; the electric spark in it, 323; iii. 470; converted into vitriolic acid, 330; the freezing of water impregnated with it, 359; do. exposed to a continued heat, iii. 526
- Volcanic fires*, how supported, 63
- Wad, black*, air from it, 76
- Water*, the state of air in it, 56; necessary to inflammable air, 266; impregnated with nitrous air, 364; necessary to nitrous air, ii. 34; necessary to the decomposition of nitrous air by iron, 98; impregnated with vitriolic acid air, 307; with fluor acid air, 342; with alkaline air, 372; with phlogisticated nitrous vapour, iii. 129; fresh distilled injures air, ii. 242; its effect on phlogisticated air, 271; its seeming conversion into air, 407; of use in fertilizing meadows, iii. 305; mercury agitated in it, 446; the quantity of marine acid air and vitriolic acid air necessary to saturate it, iii. 564
- Weights*, used in these experiments, 42
- White matter* from dephlogisticated air, ii. 178
- Willow plant*, absorbs air, iii. 331; its growth in different kinds of air, 336
- Wines*, air from them, 92

W

Z

Zinc, inflammable air from it and sulphur, 186; air from it by steam, 301; by spirit of nitre, ii. 66



E R R A T A.

Page 100, in the title of section IX. for *dephlogisticated*, read *phlogisticated*.

Page 273, in the title of section III. for *nitrated*, read *vitiated*.

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